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When Visual Marking Meets the Attentional Blink: More Evidence for Top-Down, Limited-Capacity Inhibition

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An *attentional blink* (AB) paradigm was used to investigate the attentional resources necessary for visual marking. The results showed that distractors presented inside the AB cannot easily be ignored despite participants anticipating a future target display. This supports the hypothesis that attentional resources are required for visual marking. In addition, probe dots were better detected on blinked distractors than on successfully ignored distractors, but only when the task required new items to be prioritized. In a final experiment, a stronger negative carry-over effect on search occurred for targets identical to distractors presented outside rather than inside the AB. This suggests that at least part of the inhibitory processes involved in visual marking are nonspatial.

The study of visual selective attention focuses on our visual system's ability to prioritize certain visual events over others. In brief, efficient prioritization depends on the spatial and temporal properties of, as well as the task constraints surrounding, the visual event. In the present study, we considered the interactions between these spatial and temporal factors.

Visual selective attention has a strong spatial component. Typically, visual objects relevant to our behavior (*targets*) occupy limited spatial regions in a cluttered visual field filled with numerous irrelevant objects (*distractors*) that are simultaneously present. Sometimes selection of a target is quite effortless. For instance, Treisman and Gelade (1980) found that observers were very efficient in searching for a blue *T* in a display filled with brown *T*s and green *X*s. In this single-feature search task it is as if the unique feature (color) of the target guides selection. Typically, therefore, the number of distractors (the display size) has little or no effect on search reaction times (RTs), creating flat slopes for the Display Size × RT search functions. In other tasks, selection may be more effortful. For example, Treisman and Gelade found that search for a green *T* among brown *T*s and green *X*s was much less efficient than a single-feature search. In this conjunction search, visual attention cannot be guided by the target because the target is defined only by a combination of features it shares with both distractor types. Instead, it is as if attention has to be shifted around the display in an effortful way until the target is found. Typically, therefore, conjunction-search RTs are dependent on the number of items simultaneously present, resulting in a relatively steep search slope (see Wolfe, 1994, and Wolfe, Cave, & Franzel, 1989; for variations; but see Duncan & Humphreys, 1989, for a different explanation).

Visual selective attention also has a strong temporal component. Visual objects occupy limited periods in time as they appear and disappear continuously, for example, when objects (such as cars and birds) move into our environment, when we ourselves move around and previously occluded objects emerge, or when individual objects come into existence more abruptly (such as blinking traffic lights, or a camouflaged bird jumping out of a bush). One of the interesting aspects of visual attention is its ability to anticipate the selection of future events. Anticipation of new objects may occur in several ways. One way is to create an attentional set that biases the processing of visual properties in future stimuli. For instance, Folk and colleagues (Folk, Remington, & Johnston, 1992; Folk, Remington, & Wright, 1994) found evidence that observers create a broad *attentional set* for the target properties in a visual search task in anticipation of the target's appearance. Thus, any other objects (distractors) in the visual field that also correspond to that attentional set capture visual attention, even when the target has not appeared yet. For example, Folk et al. (1992) asked participants to look for a color-defined target in a visual search display. However, just before the target emerged, a cue was presented, which, in one particular condition, was 100% invalid (i.e., it always appeared in a different position to the target). Folk et al. (1992) found that these cues had an adverse effect on target detection RTs but only if the cue was defined by color as well. When the cue was defined by an abrupt onset, no RT cost was found. The complementary effect was also found: When the target was defined by an abrupt onset, onset cues interfered with performance but color cues did not. This provides strong evidence for the ability of people to anticipate what they expect to be the target by enhancing broadly tuned visual channels important for target detection (Folk et al., 1994).

Another way to facilitate the selection of new items is to suppress the properties of current (old) stimuli through inhibitory processes. One such inhibitory mechanism, termed *visual marking*, was proposed by Watson and Humphreys (1997; see also Olivers, Watson, & Humphreys, 1999; Watson & Humphreys, 1998, 2000). Crucially, visual marking is hypothesized to be a top-down atten-

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tional process and is therefore dependent on limited-capacity resources. Such a top-down inhibitory mechanism may have several advantages. First, it allows for efficient selection of new objects whose properties are not yet known. For example, imagine we walk in a forest and suddenly hear a noise. We anticipate that soon an animal will jump out. However, this could be an animal of any kind with any combination of very different shapes, sizes, colors, and dynamic properties. It could be a black and white magpie, a red fox, a brown rabbit, or a green snake. Instead of trying to bias selection toward the unknown, selection may be optimized by suppressing the properties of the objects currently present. A second advantage of top-down inhibition is that, in combination with other attentional sets (see above), it allows for more cognitive control in selection. If the present stimuli are not relevant to us, we may suppress them, but when they are, or become, relevant, their suppression may be lifted or even turned into enhancement. Again, this control can be more precise and more effective because it is dealing with the information currently available.

In the present study we sought to test further the top-down aspects of visual marking. One rather counterintuitive prediction from the visual marking account is that if attention is not available, visual information cannot be ignored. This is because without attentional resources, no top-down inhibitory set can be created. We tested this directly by presenting the old, irrelevant information inside a so-called *attentional blink* (e.g., Raymond, Shapiro, & Arnell, 1992; Shapiro & Raymond, 1994) and then measured to what extent this old information interfered with a subsequently presented target set.

Visual Marking

Using an adapted conjunction task, Watson and Humphreys (1997) investigated the role of old distractors in visual search displays. They gave participants a 1,000-ms preview of a set of green *H* distractors before adding a second set to the display, consisting of blue *A* distractors and, on present trials, a blue *H* target. Once the second set was presented, the display conformed to that used in standard form–color conjunction tasks. However, in this preview condition, search was much more efficient than in the standard conjunction baseline, in which both green and blue distractors appeared simultaneously. In fact, search slopes were no higher than in a standard single-feature baseline, in which only the second set (the blue items) was present. Apparently, participants could use the preview period to ignore the old items and limit their search to the new items only. Thus, although the physical appearance of the distractors did not change, the fact that they appeared at an earlier moment in time reduced their influence on selection (see also Kahneman, Treisman, & Burkell, 1983). Subsequent experiments have shown that, unlike in onset capture experiments (Yantis & Jonides, 1984), the effect disappears when part of the old items is offset, indicating that the preview effect is not just caused by new onset capture. Furthermore, the preview benefit does not appear to be due to inhibition of return. For instance, a brief complete offset of the old items eliminates the preview effect (Watson & Humphreys, 1997), whereas inhibition of return has been shown to survive such transient visual changes (Klein, 1988; Maylor & Hockey, 1985; Posner & Cohen, 1984; though see Müller & von Mühlenen, 2000, and Takeda & Yagi, 2000).

Instead, Watson and Humphreys (1997) proposed a mechanism of visual marking to account for the findings. They suggested that the preview period allows observers to mark the old items by inhibiting them in parallel, and hence the new set is prioritized. Figure 1 shows a tentative model of how this might work. When the observer is facing some old and irrelevant visual information, he or she may set up a goal state to ignore the old set in order to anticipate the target set. This involves establishing an inhibitory template (or inhibitory attentional set) specifying what visual representation is to be suppressed. This visual representation may take several forms. Watson and Humphreys (1997; see also Olivers et al., 1999) proposed that the locations of to-be-ignored items are represented in a location map (cf. Treisman & Gelade, 1980), as illustrated in Figure 1. These locations are subsequently marked for inhibition so that they will be avoided in the search process. Marking locations can be quite effective, as it is precise and allows for a heterogeneous scene to be inhibited without having to specify all visual properties of that scene (e.g., Theeuwes, Kramer, & Atchley, 1998). In addition, there is evidence that visual marking can also be applied to other stimulus representations. For instance, Watson and Humphreys (1998) and Olivers et al. (1999) found evidence that with moving stimuli, inhibition is applied to a unique

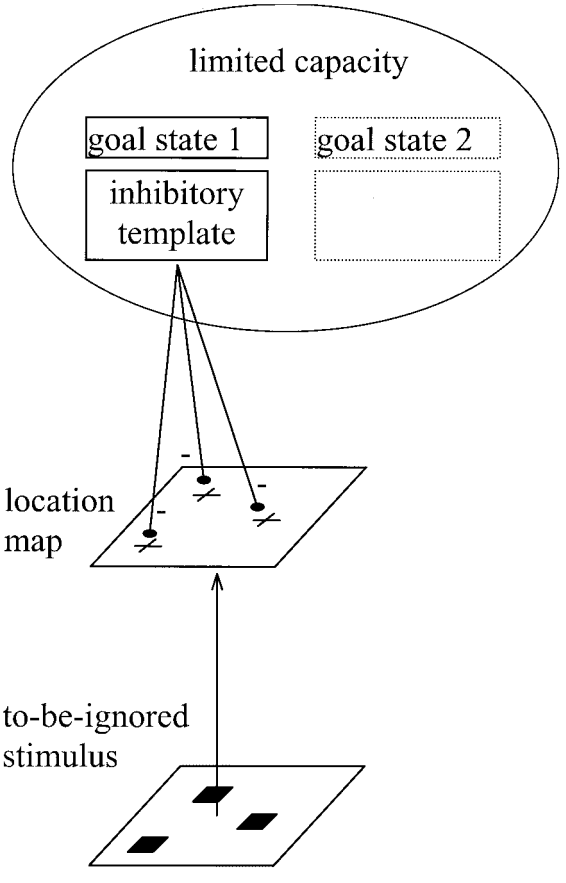

Figure 1. Working model of visual marking. Adapted from "Visual Marking: Prioritizing Selection for New Objects by Top-Down Attentional Inhibition of Old Objects," by D. G. Watson and G. W. Humphreys, 1997, *Psychological Review, 104,* p. 117. Copyright 1997 by the American Psychological Association.

feature of the old items, in this case color. So, for example, by suppressing the feature map coding green, all green items can be deprioritized, wherever they are in the visual field.

For the purposes of the present study it is important to note that Watson and Humphreys (1997) conceived of visual marking as being a top-down, goal-driven process that depends on limited-capacity attentional resources. Support for this view comes from a number of experiments. For instance, Watson and Humphreys (2000) used a probe-detection task to measure the inhibition of old distractors directly. On a majority of trials, participants carried out a search task after viewing a preview of one set of distractors. On a minority of trials, however, a tone indicated that participants had to switch tasks and detect a probe dot instead. Although the probe was equally likely to appear on a new or an old item, detection was less accurate for the latter. It was also less accurate than detection for the equivalent (green) items in a standard conjunction baseline, in which all items appeared simultaneously. This is consistent with the old items being inhibited. The most important finding, however, was that the reduced detection on old items was greatly diminished when participants had to abandon the search task completely and instead detect a probe on every trial. This provides direct evidence that the inhibition can be modulated by the task demands. Old items are less inhibited when the inhibition is actually detrimental to the task (because a dot can appear on one of them).[1]

A second example comes from an earlier experiment by Watson and Humphreys (1997; Experiment 8). In this experiment, Watson and Humphreys gave participants a secondary task during the preview period, when only the green *H*s were present. The task was to shadow a series of four digits at fixation (presented for 250 ms each), followed by the search task. The result was that the preview effect was attenuated, as search slopes increased relative to the single-feature baseline (though there was still a secondary task preceding the search displays in the baseline). Watson and Humphreys (1997) concluded that visual marking required limited attentional resources, in line with a top-down inhibitory account (see also Humphreys, Watson, & Jolicoeur, in press).

Overview of the Present Experiments

In the present study we investigated in further detail the resource limitations of visual marking. One of the most important and somewhat counterintuitive predictions from the visual marking account is that taking attention away from distractors will not help in ignoring them. To ignore distractors effectively, an inhibitory template needs to be set up, and this requires attentional resources. To test this, we sought to apply a way of removing attention from the old distractors that was somewhat different from Watson and Humphreys's dual-task experiment (Watson & Humphreys, 1997, Experiment 8; described above). Instead of presenting a task during the preview period, we looked for a task that could be conducted beforehand but that would still exert its influence during the preview period. An excellent candidate is the rapid serial visual presentation (RSVP) task, as employed in the attentional blink paradigm (e.g., Raymond et al., 1992). In a typical RSVP task, the observer is presented with a stream of letters at fixation (usually around 10 letters per second). In the dual-task condition, the observer has to first identify a differently colored target letter (T1) and then detect whether a second target letter (T2) is present in the remainder of the stream. In the single-task condition, T1 is still present but only T2 needs to be detected. The common finding is that in the dual task, the detection of T2 is severely impaired if it appears shortly after T1 (although the immediately following item is often spared) and then gradually improves with time until it is back to the single-task baseline about 400 to 500 ms after T1. It is as if attention blinks for about half a second (Raymond et al., 1992; Shapiro & Raymond, 1994).

In the current experiments, we used the RSVP task as a tool to manipulate the amount of attention available for visual marking by presenting the preview display more or less inside an attentional blink period. We found that the closer in time the previewed distractors were presented to T1, the less efficient subsequent search became, suggesting that distractor inhibition requires attentional resources.

Subsequently, we investigated how the attentional blink affects visual marking. The model outlined in Figure 1 assumes there are several processing stages to visual marking, such as the creation of a location map and the setting up of an inhibitory template. These were pitted against each other in Experiments 3 and 4. We found no evidence for the disruption of spatial processing. Instead, our results suggest that the attentional blink affects the top-down inhibition of the old distractors.

The attentional blink procedure has certain advantages over Watson and Humphreys's (1997) dual-task experiment. First, it allows for a gradual manipulation of the amount of attentional resources available for visual marking by shifting the preview display more or less into the blink period. We predicted that visual marking would be most affected when the old distractors were presented immediately after T1, with a gradual increase in performance as stimulus onset asynchrony (SOA) was increased. Second, in contrast to Watson and Humphreys's (1997) Experiment 8, in our attentional blink procedure the secondary task was presented before, instead of during, the preview display. This has the advantage that there is no low-level visual interference from the secondary task during the actual preview period. In Watson and Humphreys's (1997) experiment, any attenuation of the preview effect may have been caused by the stimulus itself rather than by the task demands (though see Humphreys et al., in press).

Experiment 1: Establishing the Preview Effect

In Experiment 1 we sought to replicate Watson and Humphreys's (1997) basic preview effect. Because we changed some

[1] As was pointed out by one of the reviewers, the evidence for inhibition is not conclusive in this case because of the difficulty of establishing an appropriate baseline. Here the baseline was a standard conjunction condition in which all items appeared simultaneously. However, it may have been that in this baseline all items were relatively enhanced (because all were new onsets). New evidence from our lab (Olivers & Humphreys, 2001b) points more directly toward suppression. We conducted experiments in which a salient new feature singleton appeared in the second set. Normally, such singletons can guide attention (Yantis & Egeth, 1999). However, we found that this guidance was mitigated when the singleton shared one or more of its features with the old, ignored distractors. This suggests an inhibitory carryover from old to new items on the basis of similarity. Similar effects were found in the present study (Experiment 4). Moreover, in Experiment 3 we found probe dot detection effects that are difficult to explain under a simple enhancement account.

aspects of the task, a replication was desired to provide a baseline for subsequent experiments, which evaluated performance under dual-task conditions. Figure 2 shows a typical trial from the preview (PV) condition in Experiment 1. We presented participants with a display of green *H* distractors, followed after 1,000 ms by the addition of a set of blue *A* distractors and a blue *H* target. As in Watson and Humphreys's (1997) study, we compared this PV condition to two baselines, namely, a standard single-feature (SF) search for a blue *H* among blue *A*s and a standard conjunction (CJ) search for a blue *H* among simultaneously presented green *H*s and blue *A*s. However, unlike in Watson and Humphreys's (1997) study, in our study the target was almost always present (save a few catch trials), and the participant's task was to localize it by way of pointing and clicking with the mouse. This led to the advantage of easier interpretation of the results, as absent trials in simple detection tasks often tend to be subject to a variety of search strategies (Corcoran & Jackson, 1979; Humphreys & Müller, 1993; Wolfe et al., 1989). Furthermore, this procedure maximized the number of critical target-present trials for a given participant. In the localization task, there is also less room for speed–accuracy trade-offs because participants know the target is (almost) always there.[2]

Method

Participants. Nineteen undergraduates (11 male and 8 female, all right-handed) from the University of Birmingham participated in return for course credits or money. The average age was 21.9 years (range = 18–35 years). All participants had self-reported normal or corrected-to-normal vision.

Apparatus and stimuli. The visual search displays were presented on a 15″ (38.1-cm) monitor driven by a Pentium-200 PC with a VESA graphics card running at 800 × 600 × 256 resolution. The stimuli were generated by a purpose-written Turbo Pascal 7.0 program, which also recorded RTs and responses. The viewing distance was approximately 75 cm. The visual search displays were constructed by randomly plotting all search items on an 8 × 8 grid, subtending approximately 8.3° × 8.3° in visual angle. The letters, *A* and *H*, were rectangular (as on a digital alarm clock) and were 0.6° high by 0.4° wide. The green and blue colors were very similar to the colors in Watson and Humphreys's (1997) original experiment and were roughly isoluminant (as determined by a flicker test on the experimenter; Ives, 1912). In the pointing display, all cells of the 8 × 8 grid were filled with gray circular position markers with a radius of about 0.2°.

Design and procedure. Each trial started with the appearance of a fixation cross, which stayed on during the remainder of the trial. After 750 ms, either a search display appeared (SF and CJ conditions) or a preview display appeared first, followed after 1,000 ms by the search display (PV condition). The search display stayed on until the participant responded, with a maximum duration of 5s. In the SF condition, displays consisted of a blue *H* target, together with 1, 3, or 7 blue *A* distractors, resulting in total display sizes of 2, 4, and 8 items, respectively. In the CJ condition, for each blue item, there was a green *H*, resulting in total display sizes of 4, 8, and 16 items, respectively.

The PV condition was the same as the CJ condition except that the green *H*s were presented first, with the blue items added only after 1,000 ms. Participants were instructed to click the left mouse button as soon as they detected the blue *H* target but to withhold their response if there was no target present. The latter was the case on 6% of trials. These catch trials were added to discourage participants from making anticipatory responses. After the first click (which was timed), the search display was replaced with a pointing display (see Figure 2), and participants had to move the mouse pointer to the target position and click again. This second click and the movements leading up to it were not timed. The main conditions were run in separate blocks, with 16 trials for each Condition × Display Size combination. Block order was completely counterbalanced across participants (apart from order CJ-PV-SF, which was run four times instead of three). Trials that led to an incorrect response (i.e., wrong localizations, responses to catch trials, RTs < 100 ms, and RTs > 5,000 ms) were repeated by randomly intermingling them with the remainder of the trials. Feedback was provided on every trial. Correct responses were followed by a tick mark at the target location as well as a brief high-pitched tone. Incorrect responses were followed by a cross mark and a longer, low-pitched tone. After every 45 trials there was a short break during which participants received feedback on their average RT and accuracy levels. They were asked to maintain an accuracy level of at least 80%. A practice block of 36 trials preceded each test block.

Results

The mean RT for each cell was calculated after incorrect trials, catch trials, and outliers had been removed. Outliers were identified by a procedure recommended by Van Selst and Jolicoeur (1994). This procedure involves the recursive elimination of RTs beyond *s* standard deviations from the mean, with *s* varying according to the number of data points in the cell (i.e., a modified recursion with moving criterion). The procedure resulted in an elimination of 3% of the data points. Mean RTs were then submitted to several analyses of variance (ANOVAs), which assessed performance in the PV condition relative to the SF and CJ baselines, with display size as a factor. Similar analyses were performed on the error data. Note that in all conditions (including the SF condition), slopes were calculated for display sizes of 4, 8, and 16, although in the SF condition, the real display sizes were 2, 4, and 8, respectively. This leads to an underestimation by 50% of the real SF slope but therefore enables a direct comparison with the PV condition, in which, if observers completely ignore the first set, the use of display sizes 4, 8, and 16 will lead to the same underestimation. The inferential statistics are not affected by this procedure (see Watson & Humphreys, 1997, for further details).

RTs. Figure 3 shows the search RTs as a function of display size for each condition. Search slopes measured 12 ms per item in the SF condition, 28 ms per item in the CJ condition, and 15 ms per item in the PV condition (measured across the total display sizes of 4, 8, and 16). Overall, responses were slowest in the CJ condition and fastest in the SF condition, with RTs for the PV condition falling in between, $F(1.6, 28.9) = 25.2$, $MSE = 19{,}187$, $p < .01$.[3] Also, RTs generally increased with display size, $F(1.1, 19.4) = 70.6$, $MSE = 18{,}517$, $p < .01$, but more so in the CJ condition than in the SF and PV conditions, resulting in a significant Condition × Display Size interaction, $F(2.3, 41.3) = 17.3$, $MSE = 5{,}547$, $p < .01$. Separate ANOVAs revealed that the search slope in the PV condition was significantly more shallow than that in the CJ condition, $F(1.3, 24.3) = 15.6$, $MSE = 5{,}973$, $p < .01$, and was no different from that in the SF condition, $F(1.4, 25.0) = 2.6$, $p > .10$.

Errors. Table 1 shows the error percentages for each condition and display size. An ANOVA revealed an effect of display size

[2] The data presented in Experiment 1 formed part of a larger experiment comprising several more conditions. Here we present only the conditions relevant to the present study.

[3] Where degrees of freedom are fractionated, a Greenhouse–Geisser correction for sphericity violations was applied.

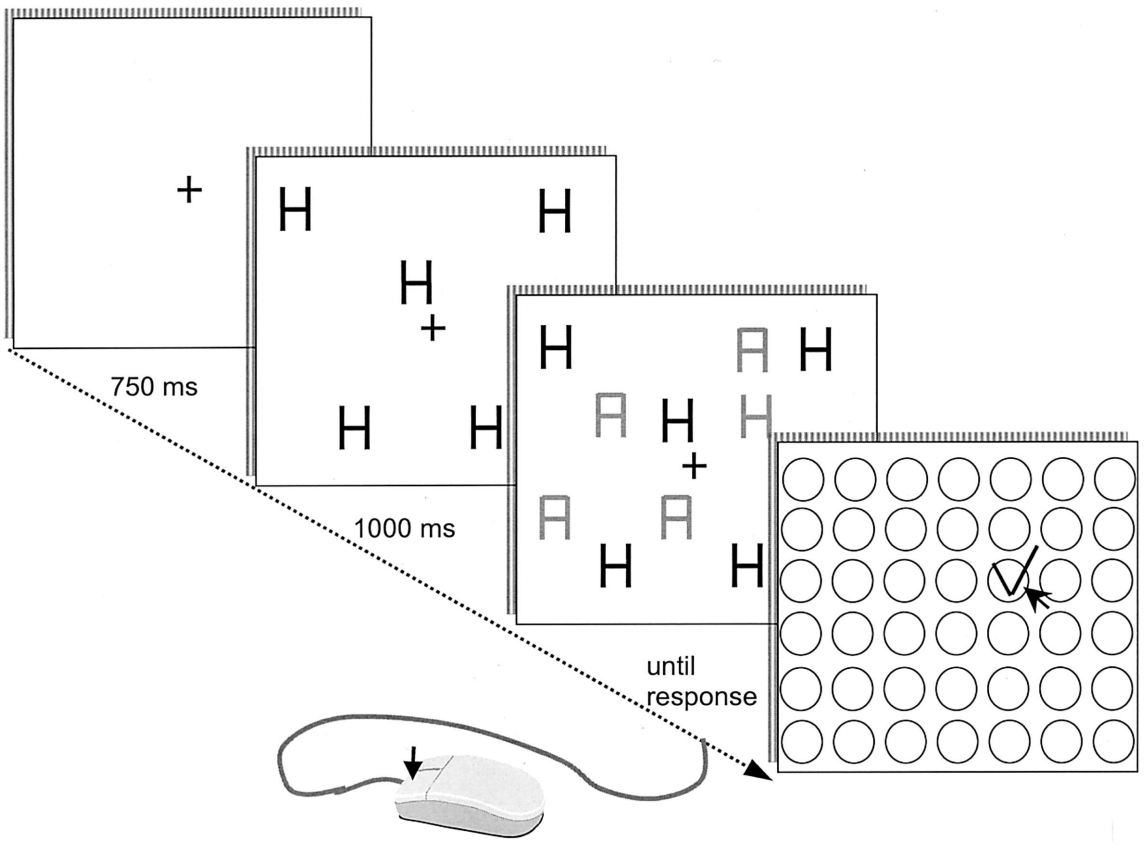

Figure 2. A typical trial of the preview condition in Experiment 1. Participants ignored the green *H*s (black) in the preview display and pressed a mouse button when they spotted a blue *H* (gray) in the second display. Reaction times were measured according to this first click. Participants then selected the target's location with the mouse pointer and clicked again. This last click was not timed.

only, $F(1.5, 27.6) = 4.9$, $MSE = 5$, $p < .05$, as errors increased with the number of blue items. The overall correlation between RTs and error percentages was .74. Close inspection of Table 1 suggests that, overall, more errors were made in the CJ and PV conditions than in the SF condition, although this was not significant. More important, display size had no differential effect across conditions, suggesting that accuracy was not sacrificed to maintain search efficiency. The average false alarm rate on catch trials was 18.5%, with an approximately equal distribution across conditions (SF: 18.5%; CJ: 22.2%; PV: 16.7%).

Discussion

The most important finding of Experiment 1 was the replication of the preview effect. The results show that performance (in terms of search slopes) in the PV condition (15 ms per item) was much more efficient than performance in the CJ condition (28 ms per item) and was almost as efficient as performance in the SF condition (12 ms per item). This indicates that old distractors can be effectively ignored and that search is limited to new items only. The fact that we used a target localization task instead of a present–absent detection task does not alter this conclusion. This therefore provides a sound frame of reference for the subsequent experiments we conducted in this study, all of which involved the localization task. Moreover, it indicates that visual marking is a robust phenomenon, emerging across a variety of tasks, and that it can be of real relevance outside the laboratory, where targets need to be localized as well as detected.

Experiment 2: The Attentional Blink Affects Visual Marking

In Experiment 2 we investigated the resource limitations of visual marking by introducing an RSVP task prior to the visual search display. A typical sequence of events is illustrated in Figure 4. In all conditions, participants first viewed, at fixation, a stream of rapidly changing letters, one of which was a target letter (T1, as defined by a different color). The letter series was then immediately followed by the preview display (consisting of green *H*s), which was presented for 450 ms. In the single-task condition, the letter stream had no relevance to the participant. In the dual-task condition, however, participants had to identify T1, which was presented at different intervals (SOAs) from the preview display. As in Experiment 1, the preview display was followed by a search display, and participants had to localize a blue *H* as quickly as possible. Finally, they were asked to type in T1 (dual-task condition only).

We predicted that T1 would cause an attentional blink, which, when triggered moments before the preview display, takes attentional resources away from visual marking. As old distractors will then receive less inhibition, subsequent search through the new set should suffer. In contrast, in the single-task baseline, inhibition should remain relatively intact and SOA should have no effect. In short, we predicted an SOA × Task interaction. This was tested in Experiment 2c, which applied the attentional blink procedure to a PV condition.

We chose to shorten the preview period from a typical 1,000 ms to 450 ms to optimize any secondary task effects. Watson and Humphreys (1997) investigated the time course of visual marking and found that the preview effect reaches its optimum around 400–600 ms. The preview period of 450 ms in the present experiment should thus have been sufficient to create at least a substantial (if not complete) preview advantage. On the other hand, attentional blink studies have shown that, at least for normal observers (cf. Husain, Shapiro, Martin, & Kennard, 1997), the attentional blink lasts between 400 and 500 ms (Duncan, Martens, & Ward, 1997; Raymond et al., 1992; Shapiro & Raymond, 1994). Therefore, for visual marking to be affected, the attentional blink period should cover a substantial part of the preview period. For instance, had we left the preview period at 1,000 ms, a 500-ms blink would still have left a sufficient period of 500 ms for marking to be implemented.

Notice that there is a confound in this procedure. Because the preview period remained fixed at 450 ms, shifting the old distractors nearer to T1 meant that the new (to-be-searched) items came equally near. Any effect of SOA may thus have been due to the attentional blink carrying over to the search stage itself instead of being restricted to the visual marking stage (assuming that the attentional blink would extend beyond 450 ms). We therefore conducted Experiments 2a and 2b, which served as control experiments, to see whether the blink extended beyond the preview period and had a systematic effect on the search stage. In these experiments also, T1 was followed by green *H*s but these had no predictive value, as they either randomly changed position (the CJ condition, Experiment 2b) or disappeared altogether (the SF condition, Experiment 2a) when the new, blue items appeared. Because we predicted that the blink would affect only the preview display, we expected there would be no SOA × Task interaction. In addition, the SF and CJ conditions of Experiments 2a and 2b served as useful baselines against which the magnitude of the secondary task interference in Experiment 2c (PV condition) could be estimated.

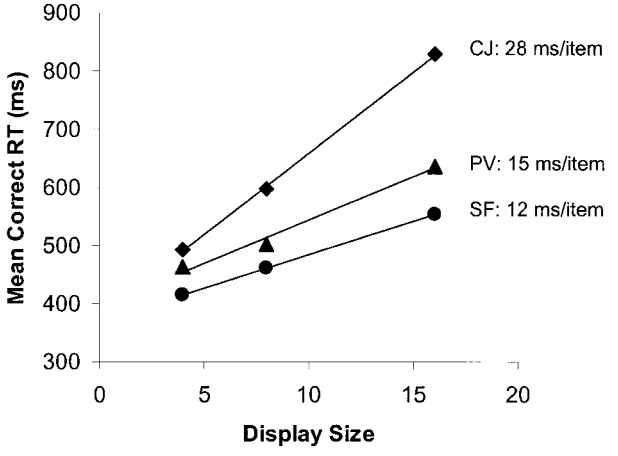

Figure 3. Search functions for Experiment 1. RT = reaction time; CJ = conjunction; PV = preview; SF = single feature.

Table 1
Error Percentages for Experiment 1

Condition	Display size (no. of items)		
	4	8	16
Single-feature search	2.0	2.2	4.0
Conjunction search	2.2	1.9	5.6
Preview	3.4	2.9	6.6

Method

Participants. Forty-eight (6 male and 42 female, including 2 left-handed and 46 right-handed) undergraduates and postgraduates participated voluntarily, for course credits or for money. The average age was 19.7 years (range = 18–26 years). Experiments 2a, 2b, and 2c each had 16 participants.

Apparatus and stimulus. The experimental setup was the same as in Experiment 1. The visual search displays were also identical. The new part of the study was the introduction of a rapidly presented series of letters appearing prior to the search display. The series always began and ended with an asterisk, which served as a fixation point as well as a mask for T1 (there is evidence that the attentional blink is abolished if T1 is not masked; see Breitmeyer, Ehrenstein, Pritchard, Hiscock, & Crisan, 1999; Raymond et al., 1992; Seiffert & Di Lollo, 1997). The letters were randomly drawn from the alphabet (with the restriction that two consecutive letters could not be identical) and presented in a light blue 24-point Helvetica font (approximately 0.4° × 0.4°). The target (T1) was yellow. This color scheme was chosen because in a pilot study with the typically used black distractors and a white T1, there appeared to be an attentional blink even in the single-task condition, suggesting that a T1 that contrasts too much with the distractors might automatically capture attention in our setup. The letters were presented at a rate of 8.5 Hz, with each letter being presented for 100 ms, followed by a 17-ms blank.

Design and procedure. The SF, CJ, and PV conditions were run separately in Experiments 2a, 2b, and 2c, respectively, each with a new group of participants. In all experiments and all conditions, trials started with a blank screen lasting 750 ms, followed by a fixation asterisk for 500 ms, which was in turn followed by a series of letters. The length of the series varied randomly between 14 and 20 letters, to prevent anticipation. The series ended with another asterisk, masking the last character. Counting from the end of the series, the target letter, T1, appeared at Positions 1, 2, 3, 5, and 8, with Position 1 being the last character before the asterisk. These positions corresponded to SOAs of 117, 234, 351, 585, and 936 ms, respectively. After the letter series, and simultaneously with the final asterisk, a preview display consisting of green *H*s appeared in all three conditions. The preview period lasted 450 ms. In the SF condition (Experiment 2a), the preview display simply disappeared and was replaced with a search display consisting of only blue items, with the restriction that none of the blue items could fall on an old location. In the CJ condition (Experiment 2b), the preview display also disappeared when the search items arrived but was immediately replaced with a new set of randomly positioned green *H*s, as well as the blue target set. Again, none of the new items could fall on an old item's location. In the PV condition (Experiment

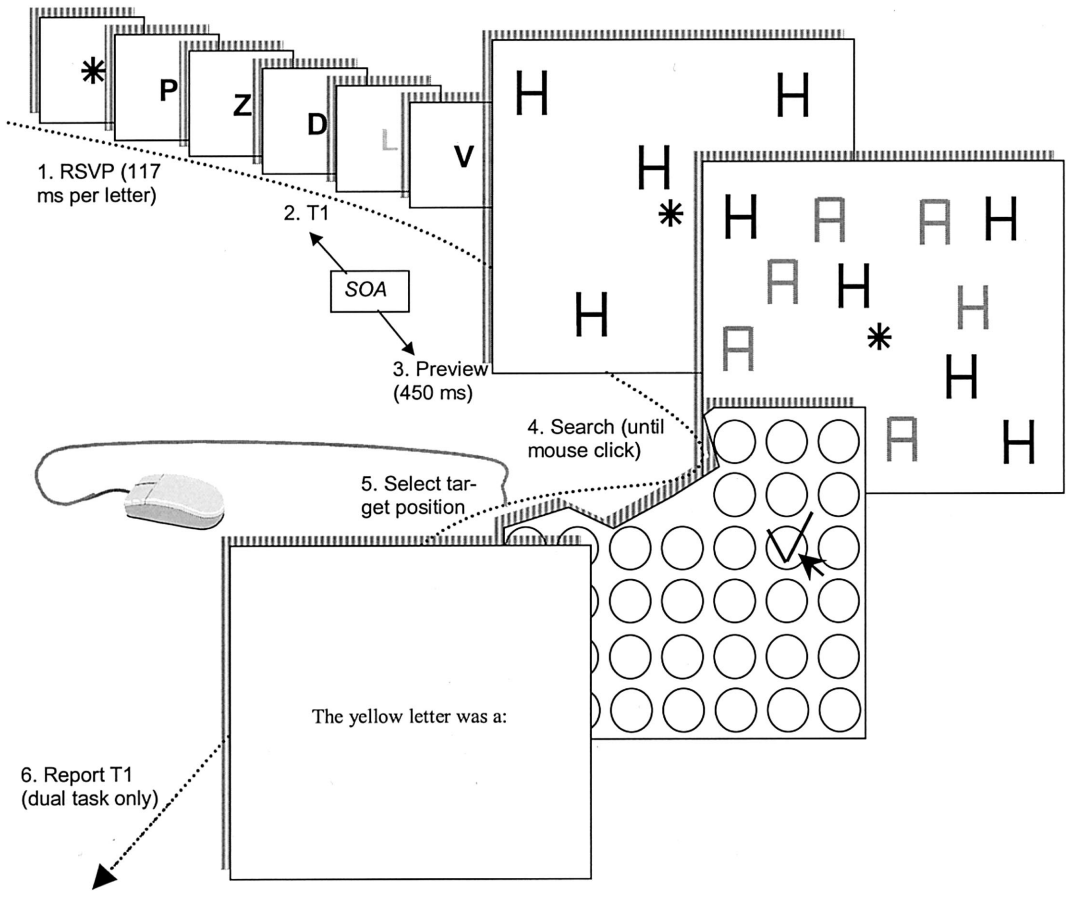

Figure 4. A typical trial from Experiment 2: The figure shows a rapid serial visual presentation (RSVP) task combined with a preview display, as it would occur in the preview condition (Experiment 2c). The RSVP letters were blue, except for the first target letter (T1), which was yellow. The stimulus onset asynchrony (SOA) was the time between the onset of T1 and the onset of the preview display. The search task was the same as in Experiment 1. In the dual-task condition, search was followed by a T1 response display.

2c), the preview display was a genuine preview display, as the green *H*s remained in their positions when the second display appeared.

The search task was the same as in Experiment 1 but with total display sizes of 6 and 12 items, half of which were green and half of which were blue. This time, 12% of the trials were catch trials, in which case participants were instructed not to respond. In the single-task condition, participants were asked to ignore the letter stream, while maintaining fixation, and to concentrate on the search task. In the dual-task condition, participants were asked to extract T1 from the letter stream and remember it until the end of the trial. After they had correctly localized the search target (blue *H*), they were asked to type in the letter they saw (this was not timed). The search task was stressed as the most important task, but participants were encouraged to identify as many letters as they could and to guess if they were not sure. If they made an error in the search task, they were not asked to identify T1.

Each experimental session followed an ABAB design with single task and dual task alternating between blocks. Block order was counterbalanced between participants. Participants received feedback on the search task as well as on the T1 identification task. After every 45 trials there was a short break in which participants were presented with their overall RTs and accuracy scores. Erroneous trials were repeated by being randomly inserted in the remainder of the block. At the end of the experiment there were 15 correct trials for each combination of task, SOA, and display size. Because it was a complex and difficult procedure, the experiment was preceded by an extensive practice session. Participants first practiced T1 detection only. After the letter task, participants practiced the search task only, followed by a block of 24 trials in which the two tasks were combined. At this stage, participants generally no longer had difficulties with the task, although we replaced 5 of the 48 participants because they made errors on more than 30% of the trials in either of the tasks.

Results

The RT data were submitted to the same outlier elimination procedure used in Experiment 1. This resulted in a loss of 2.8% of the data points in Experiment 2a, 2.3% in Experiment 2b, and 2.4% in Experiment 2c. Remaining correct RTs (excluding trials on which T1 was not identified correctly) as well as error percentages were then submitted to within-subjects ANOVAs with task (single, dual), SOA (117, 234, 351, 585, 936 ms), and display size (6, 12) as factors. The descriptive RT data of Experiment 2 are shown in Figures 5–8. Table 2 shows the error percentages for the search tasks (collapsed across display sizes) as well as for the T1 detection task. We report the analyses for each condition sepa-

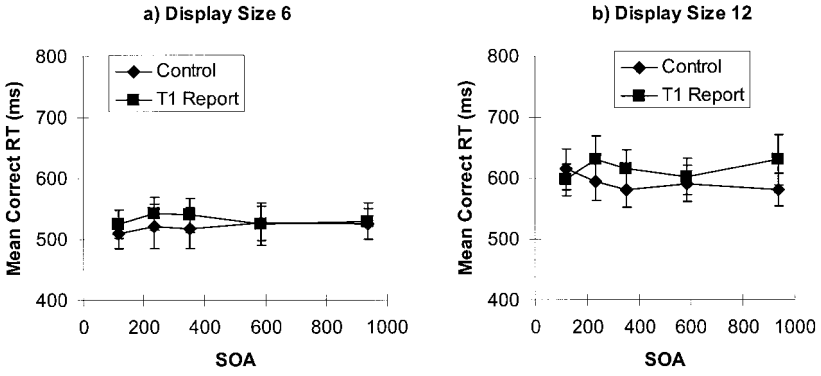

Figure 5. Mean correct reaction times (RTs) for Experiment 2a (single-feature condition) for (a) Display Size 6 and (b) Display Size 12. Error bars represent one standard error. T1 = first target letter; SOA = stimulus onset asynchrony.

rately before we move on to the between-subjects analyses across the search conditions of Experiments 2a through 2c.

The catch trials resulted in 14.5% false alarms. This is lower than in Experiment 1, probably because of the higher proportion of catch trials present (11.7% vs. 6.3%). Overall, false alarms were evenly distributed across task, display size, and condition, although more errors were made in the SF condition (17.2%) than in the CJ and PV conditions (12.2% and 13.9%, respectively).

Results: Experiment 2a (SF Condition)

RTs. Search RTs are shown in Figure 5. The only significant main effect was of display size, $F(1, 15) = 83.6$, $MSE = 5{,}821$, $p < .01$. It took longer to search 12 items than to search 6, resulting in an average slope of 13 ms per item (collapsed across SOA and task). This slope is almost identical to the SF slope found in Experiment 1 (12 ms per item). Task and SOA had no effect (Fs $<$ 1). Most important, there was no Task $\times$ SOA interaction, $F = 1.2$, nor was the Task $\times$ SOA $\times$ Display Size interaction significant, $F = 1.76$. As can be seen from Figure 5, RTs remained reasonably stable across SOAs in the single- as well as the dual-task conditions. Although the ANOVA results gave us no reason to do so, we performed separate *t* tests on the single- versus dual-task conditions for each SOA and display size (Fisher's least significant difference [LSD]) as well as *t* tests between the last SOA (936 ms) and each of the other SOAs in the dual task. We did this to match the analyses with those in Experiment 2c, in which such analyses were warranted by the ANOVA. The individual *t* test, being more powerful, may detect a residual task effect. However, none of the *t* values was significant.

Errors. A similar ANOVA revealed no significant main effect of error percentages and no interactions. Error levels remained virtually identical across SOAs and display size. Again, we performed separate *t* tests for each SOA so that statistical power would be matched across Experiments 2a through 2c. Table 2 suggests a trend for more errors in the dual-task condition at the shortest SOA (117 ms), and for Display Size 6 this proved to be significant, $t(15) = -2.2$, $p < .05$.

Results: Experiment 2b (CJ Condition)

RTs. Search RTs, as a function of task, SOA, and display size, are shown in Figure 6. Again, the only significant factor was a main effect of display size, $F(1, 15) = 131.7$, $MSE = 19{,}235$, $p < .01$. RTs were slower for Display Size 12 than for Display Size 6, and the average search slope (collapsed across SOAs and task)

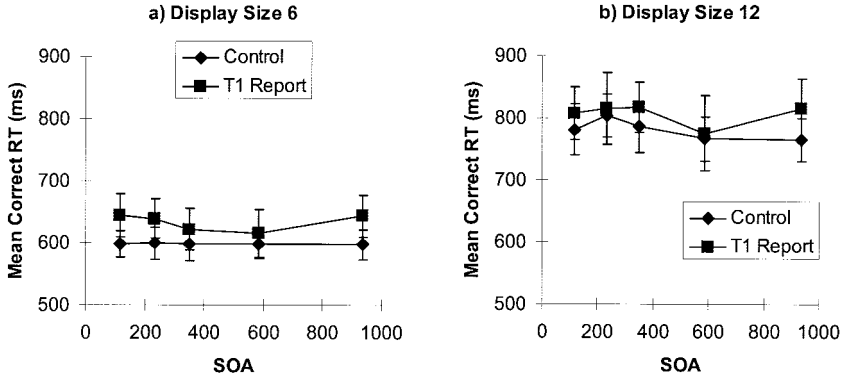

Figure 6. Mean correct reaction times (RTs) for Experiment 2b (conjunction condition) for (a) Display Size 6 and (b) Display Size 12. Error bars represent one standard error. T1 = first target letter; SOA = stimulus onset asynchrony.

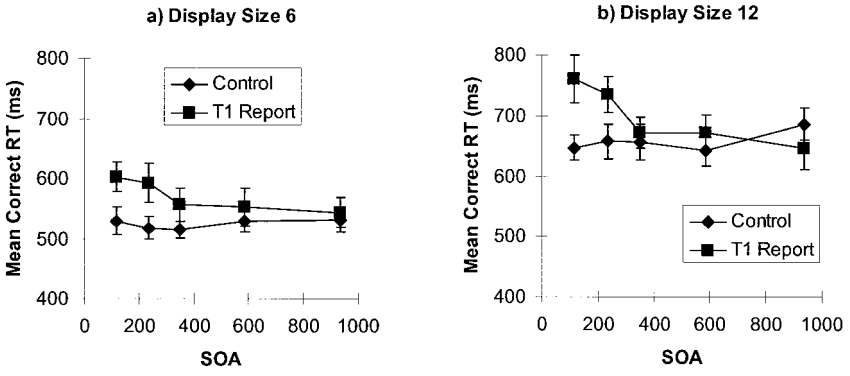

Figure 7. Mean reaction times (RTs) for Experiment 2c (preview condition) for (a) Display Size 6 and (b) Display Size 12. Error bars represent one standard error. T1 = first target letter; SOA = stimulus onset asynchrony.

measured 30 ms per item. This is comparable to the CJ slope of Experiment 1 (28 ms per item). Search times remained unaffected by task or SOA, as was further confirmed by individual *t* tests.

Errors. The ANOVA on error percentages revealed no main effects or interactions, and also Table 2 suggests that error percentages remained reasonably constant. However, a *t* test indicated a significant difference in errors between the shortest and longest SOAs in the dual-task condition (117 vs. 936 ms) on Display Size 6, with more errors being made on the shortest SOA. This result is similar to that obtained in Experiment 2a, although there the effect was significant only relative to the single-task baseline condition.

Results: Experiment 2c (PV Condition)

RTs. As can be seen in Figure 7, the pattern of performance in Experiment 2c was quite different from Experiments 2a and 2b. As before, RTs were slower overall in the dual-task condition, $F(1, 15) = 6.9$, $MSE = 20{,}703$, $p < .05$, and increased with display size, $F(1, 15) = 250.8$, $MSE = 5{,}453$, $p < .01$. The average search slope was 22 ms per item. An interesting finding was that RTs increased at shorter SOAs, $F(3.4, 50.5) = 4.6$, $MSE = 3{,}923$, $p < .01$. Figure 7 indicates that this was confined to the dual-task condition only, resulting in a significant Task × SOA interaction, $F(3.3, 49.7) = 9.6$, $MSE = 3{,}046$, $p < .01$.

We performed individual *t* tests (Fisher's LSD) comparing the dual- with the single-task condition for each SOA and display size to explore the dual-task interference across time. At Display Size 6, the dual-task condition led to significantly increased RTs for SOAs of 117, 234, and 351 ms: $t(15) = 3.1$, $p < .01$; $t(15) = 2.9$; $p < .05$; $t(15) = 2.2$, $p < .05$, respectively. At the longest two SOAs, 585 and 936 ms, there were no reliable differences between tasks ($ps > .23$). At Display Size 12, the dual-task condition led to significantly increased RTs for SOAs of 117 and 234 ms, $t(15) =$

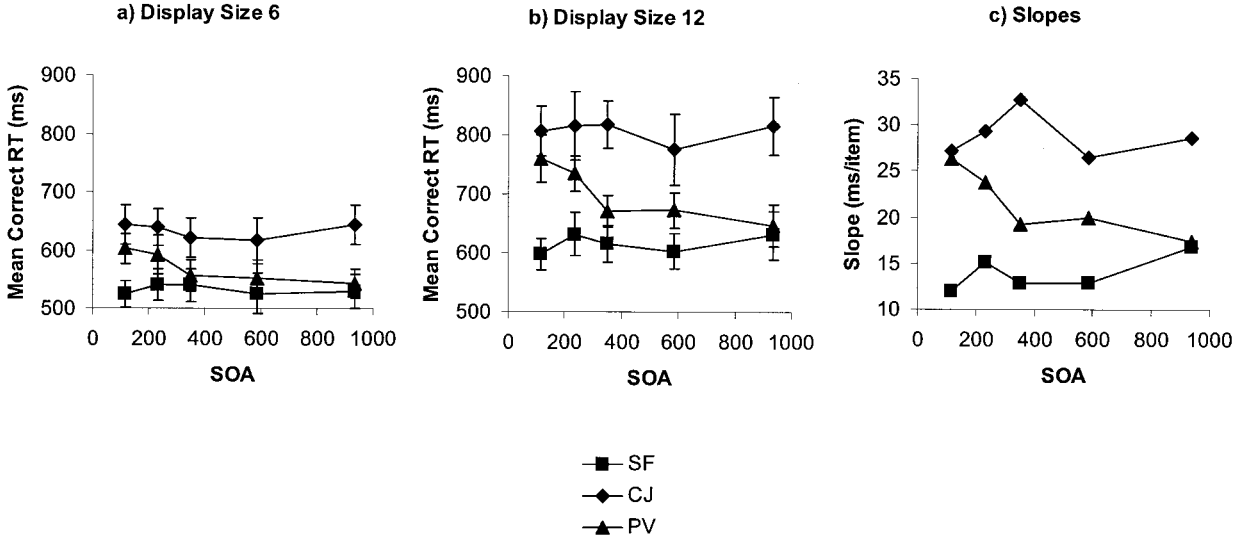

Figure 8. Combined results of all dual-task conditions of Experiment 2. (a) Reaction times (RTs) for Display Size 6, (b) RTs for Display Size 12, and (c) search slopes across display sizes. Error bars represent one standard error. SOA = stimulus onset asynchrony; SF = single feature; CJ = conjunction; PV = preview.

Table 2
Error Percentages for Experiment 2

Task	SOA (ms)	Error percentages		
		Expt 2a: SF	Expt 2b: CJ	Expt 2c: PV
T1 detection		14.0	10.6	10.2
Single task	117	3.1	3.0	4.4
	234	4.0	3.8	4.3
	351	3.7	4.4	2.2
	585	4.2	3.7	4.2
	936	3.7	4.6	2.1
Dual task	117	4.8	4.8	4.1
	234	3.1	3.0	3.7
	351	4.2	3.7	3.6
	585	3.8	3.9	2.8
	936	4.8	2.7	4.4

Note. SOA = stimulus onset asynchrony; Expt = experiment; SF = single-feature search; CJ = conjunction search; PV = preview; T1 = first target letter.

4.4, $p = .001$, and $t(15) = 2.47$, $p < .05$, respectively, but not for SOAs of 351, 585, and 936 ms ($ps > .17$). As can be seen from Figure 7, the RT differences between the dual and single task became gradually smaller with increasing SOA.

Because dual-task performance never reached the same level as single-task performance (except on Display Size 12, SOA 936 ms; see also Experiments 2a and 2b), the single task may not be the most appropriate baseline against which to compare the effects of SOA. We therefore also performed *t* tests within the dual task only, comparing the first four SOAs (117, 234, 351, 585 ms) to the last SOA (936 ms). This led to roughly the same picture, with significant differences for the shortest two SOAs (117 and 234 ms) on both display sizes, Display Size 6: $t(15) = 3.2$, $p < .01$, and $t(15) = 3.5$, $p < .01$, for SOAs of 117 and 234 ms, respectively; Display Size 12: $t(15) = 4.4$, $p = .01$, and $t(15) = 2.8$, $p < .05$, for SOAs of 117 and 234 ms, respectively. However, there were no differences for the longest three SOAs (351, 585, and 936 ms), all $ps > .2$.

Errors. The ANOVA revealed no error effects, and the *t* tests indicated a significant drop in errors in only the single-task condition on the longest SOA (936 ms), $t(15) = 3.1$, $p < .01$.

Experiments 2a, 2b, and 2c Combined: Between-Subjects Analysis Comparing SF, CJ, and PV Conditions

We combined the search data of the dual-task conditions of Experiments 2a, 2b, and 2c into an ANOVA with search condition (SF, CJ, PV) as a between-subjects factor. Figures 8a and 8b show the RTs for all dual-task conditions combined in one graph. Furthermore, in Figure 8c we plotted the search slopes for each condition and SOA. Overall, search performance was worst in the CJ condition and best in the SF condition, with the PV condition falling in between, $F(2, 45) = 5.1$, $MSE = 166{,}639$, $p = .01$. What is immediately striking in all plots is the *S* shape: With increasing SOA, performance in the PV condition changes from a level close to that of the CJ condition to a level close to that of the SF condition. This was confirmed by a reliable Condition × SOA interaction, $F(7.0, 157.6) = 5.7$, $MSE = 3{,}720$, $p < .01$. Although Figure 8c suggests a similar interaction in terms of search slopes, the Condition × Display Size × SOA effect was not reliable, $F(6.9, 155.4) = 0.9$, *ns*. Slope values tend to be more sensitive to RT fluctuations as they are calculated by using two RTs—one from Display Size 6, and one from Display Size 12. For instance, a small RT increase on Display Size 12 accompanied by an equally small RT decrease on Display Size 6 results in a double increment in slope. However, the fact that the slope pattern goes in the same direction as the absolute RT data is encouraging.

Discussion

In Experiment 2 we investigated whether visual marking is affected by the attentional blink. To do this, we first had to make sure that the attentional blink would affect only the first stage of the visual search displays. This was done in Experiments 2a and 2b. The secondary task had no significant effect on RTs, and most important, there was no effect of SOA. This suggests that the blink was over before participants started searching the second set of items. Nevertheless, a few remarks are in order. First, although the effect of task was nonsignificant in both the SF and the CJ conditions, there was a small overall trend toward slower RTs in the dual-task condition, suggesting that task load did have a general effect on search, at least on some trials. Possibly, having to remember T1 takes up attentional or mnemonic resources required for visual search. Alternatively, participants may, on some trials, still be deciding which letter they saw and hence may be less concentrated on the search task. The important point is that this was a general effect present across conditions and not modulated by SOA.

Second, participants tended to make slightly more errors on the shortest SOAs in the dual-task conditions of the SF and CJ conditions. This suggests that there may, on some trials, have been a direct influence carried over from the T1 detection task on the search task. This may have been caused by an extended blink period. Alternatively, at shorter SOAs participants may be more likely to still be deciding which letter they saw. In any case, these effects were minor and contrasted clearly with the PV condition of Experiment 2c, to which we turn next.

Search performance in the dual-task condition of Experiment 2c—the PV condition—was clearly affected by SOA. RT costs relative to the single-task condition dropped from as much as 114 ms at the shortest SOA to around 20 ms at long SOAs. The same pattern was found for search efficiency, with slopes dropping from around 26 ms per item to 17 ms per item. Note again that, as in Experiments 2a and 2b, dual-task performance never actually reached the level of the single-task condition, probably because of the reasons mentioned earlier. Nevertheless, inspection of Figure 7 suggests that the dual-task interference became negligible around on SOA of 351 ms, a finding that was further supported by the individual *t* tests. This is very close to the time course typically associated with the attentional blink. Thus, we conclude that T1 created an attentional blink, which affected subsequent processing. Because the only difference between the PV condition and the SF and CJ conditions, in which we found no blink effect, was that participants were given a useful preview of the distractors in the PV condition, we must conclude that the attentional blink affected the preview stage. This is consistent with the idea that visual marking requires attentional resources. The inhibition of irrelevant visual information is a limited-capacity process, and draining this

capacity with a secondary task makes it difficult to subsequently ignore distractors. When resources gradually become available, marking becomes more efficient.

As can be seen in Figure 8c, although visual marking did become more efficient with longer SOAs, the search slopes never reached the level of the SF condition (even though both were dual-task conditions). An exception is the longest SOA (936 ms), but this seems more attributable to the fact that the SF slope inexplicably went up instead of the PV slope coming down. Averaged across the three longest SOAs, slopes measured 29 ms per item in the CJ condition and 14 ms per item in the SF condition. These values are again very much comparable to the equivalent conditions of Experiment 1 (28 ms per item and 12 ms per item, respectively), suggesting that overall search efficiency was not hindered by the presence of a secondary task. In the PV condition, however, slopes hovered around 19 ms per item at the last three SOAs. This suggests that even after the blink period, participants could not completely limit their search to the blue items and that visual marking was therefore not fully implemented. One reason could be that the mental load of T1 affects the efficiency of visual marking. However, in the single-task condition of Experiment 2c (without T1 load), search slopes were no better (even slightly worse: $M = 22$ ms per item), suggesting that mental load was not the major cause. Instead, we believe that visual marking may have been suboptimal because the preview period (450 ms) was too short. Although Watson and Humphreys (1997) concluded that visual marking can be implemented within about 400 ms, the present task and stimulus conditions may require a somewhat longer preview period.

In Experiments 3a and 3b we explored and exploited the relationship between visual marking and the attentional blink further. By asking where in the visual process the attentional blink affects visual marking, we can learn about the mechanisms involved in both phenomena.

Experiment 3: The Attentional Blink Affects the Inhibitory Stage

In Experiments 3a and 3b we focused on the question of how the attentional blink disrupts visual marking. Incidentally, the results of these two experiments may also be useful for understanding the attentional blink phenomenon itself (see the General Discussion section). To guide the experiments, we required a working model of visual marking. We began with the model outlined in the introduction (see Figure 1). The model consists of roughly two stages. The first stage involves building up a representation of the attributes of to-be-marked distractors, such as their orientation, color, and locations. For instance, with motion displays (e.g., where all items move at constant speed down the screen), there is evidence that visual marking is applied to color maps, and this inhibition spreads to new items sharing the same color (Olivers et al., 1999; Watson & Humphreys, 1998). In contrast, other work has indicated that with static displays like the present ones, locations may play a special role in visual marking. For instance, static stimuli can be inhibited regardless of their color, orientation, or identity, and the inhibition does not spread to new items sharing the same features (Olivers et al., 1999; Theeuwes et al., 1998; Watson & Humphreys, 1997; see also Watson, 2001, for evidence that spatial layout is crucial even in motion stimuli). We hypothesized therefore, that a crucial stage within the visual marking model may be the construction of a location map (cf. Treisman & Gelade, 1980) to which the inhibition can subsequently be applied. We therefore call this first stage the *spatial encoding stage*.

The second stage is the inhibitory process itself. An inhibitory template is set up, which suppresses the locations represented in the location map. Any items at inhibited locations will suffer from a disadvantage in the competition for visual selection. We propose that the inhibitory template is maintained by the observer's goal state and is therefore subject to task constraints and limited attentional resources. We term this the *inhibitory stage*. Both the spatial encoding stage and the inhibitory stage may be affected by the attentional blink.

The literature provides little guidance on the role of the attentional blink on spatial coding. Some studies have manipulated the spatial locations of the items in an RSVP stream and demonstrated that the attentional blink extends across space. For instance, Visser, Zuvic, Bischof, and Di Lollo (1999) found that the identification of T2 was impaired even though it was presented one degree to the left or right of T1. They argued that attention cannot be switched to a new location while the system is processing T1. A similar result was obtained by Duncan et al. (1997). They presented participants with four RSVP streams arranged in a diamond so that two streams were arranged to the left and the right of fixation (horizontal streams) and the other two streams were above and below fixation (vertical streams). In one condition, participants had to detect T1 from the horizontal streams and then switch to the vertical streams to detect T2. Like Visser et al., Duncan et al. found that T2 detection was impaired at shorter SOAs. Finally, Joseph, Chun, and Nakayama (1997) showed that a T1 detection task at fixation subsequently interferes with visual search for an orientation-defined target in more eccentric locations, again showing that the attentional blink spreads beyond the location of T1. However, in all these studies, the to-be-detected T2 stimulus property (e.g., identity or orientation) was essentially nonspatial and thus did not provide a direct test for the role of the attentional blink in spatial coding.

A second way in which the attentional blink could disrupt visual marking is by affecting the inhibitory stage. According to this inhibitory account, the attentional blink leaves the spatial representation of the old distractors intact but affects the amount of resources available for suppressing them. In other words, the spatial representation stays active and cannot be ignored. To our knowledge, no study has investigated the effect of the attentional blink on inhibitory processes yet.

Finally, the attentional blink may affect both stages of our visual marking model. The fact that attentional blink effects are measured with such a widespread range of tasks (absence–presence detection, identification, visual search; e.g., Joseph et al., 1997; Raymond et al., 1992) and an equally widespread range of stimulus properties (letters, digits, basic shapes, color, orientation, motion, pictures of objects; e.g., Joseph et al., 1997; Krope, Husain, & Treue, 1998; Ross & Jolicoeur, 1999; Shapiro, Arnell, & Drake, 1991) suggests that it disrupts many different processes in the visual system. Of course, and quite likely, the omnipresent disruption may have a central origin. For example, the locus of the attentional blink may lie in the pool of limited-attentional-capacity resources (cf. Chun & Potter, 1995). Any process affecting the central capacity also affects all other processes relying on that

capacity (Lee, Koch, & Braun, 1999). Within our model of visual marking, for instance, a reduction of attentional capacity may reduce the amount of visual short-term memory (VSTM) available for distractor locations, as well as reduce the actual inhibition of those locations. In turn, both reductions may stem from the failure to set up an appropriate task–goal state in the first place, as the observer is still too involved in complying with the goals of the T1 detection task.

The basic procedure of Experiment 3 is shown in Figure 9. In Experiment 3a, we used an RSVP task (T1 identification) followed by a PV condition in which the task was to localize a blue *H*. However, we added a third task, which replaced the search task on 20% of the trials. This third task was signaled by a short beep and involved the localization of a small probe dot appearing together with the new set of items. The probe could appear either on an old item or on a new item, and participants had to press the mouse button and point to its location in the same manner as they would click and point to the blue *H* target in the majority of trials. This technique is highly similar to that used by Watson and Humphreys (Watson and Humphreys, 2000; see also Klein, 1988, and the introduction to the present article), with the important difference being that here participants had to localize the probe dot explicitly (instead of just detecting it). Also, here we used RT as the most important measure, whereas Watson and Humphreys (2000) focused on response accuracy. Watson and Humphreys showed two important results with this procedure: First, probe detection was less accurate on green items than on blue items in both the PV condition and the CJ baseline (again, in the PV condition, the green items were presented first, and were thus old). However, the difference was much bigger in the PV condition than in the CJ condition, with probe detection being especially impaired on old, green items. This strongly suggests that the old items were inhibited, consistent with visual marking. The second important finding was that the inhibition appeared to be task dependent. When observers were asked to abandon the search for the blue *H* target and to detect a probe dot on every trial, there was no difference in accuracy levels between the PV and CJ conditions. Apparently, observers inhibit old information only when it helps to perform the task, leading to the conclusion that visual marking is a top-down, goal-driven mechanism. To test this idea further, we conducted Experiment 3b, in which, just as in the Watson and Humphreys (2000) study, the dot appeared on every trial, again either on an old or a new item. On these trials, participants did not have to look for a blue *H* target, and thus we predicted there would be no inhibition

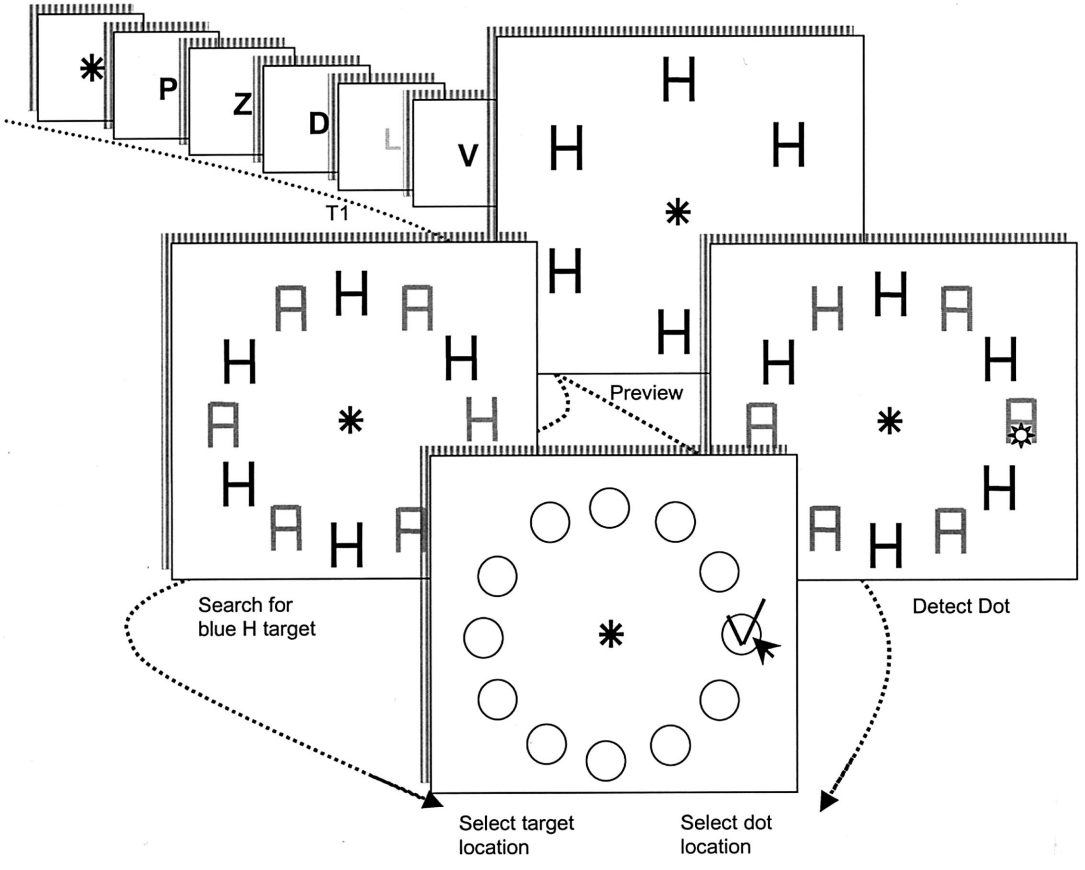

Figure 9. Part of the procedure in Experiment 3. In Experiment 3a, on most trials the search procedure was similar to Experiments 1 and 2 (left path). On a minority of trials, however, participants heard a beep just before the search display appeared, signaling that they had to localize a dot instead (right path). After the localization task, participants had to type in the first target letter (T1). In Experiment 3b, observers never had to detect a letter target and always looked for the dot instead (right path). Note that the displays were circular.

of old distractors. Experiment 3b also served as a control experiment to see whether any probe detection effects were due to differences in timing in the blink and nonblink conditions or due to low-level stimulus effects (e.g., lateral masking, when probes appeared on old vs. new items).

The probe detection paradigm led us to the following predictions:

1. Under successful marking, probe dot detection should be generally slower on old, green items than on new, blue items because the old items are being inhibited (Experiment 3a). There should be no such difference, or at least a smaller one, when observers look for a dot on every trial because dots are just as likely to appear on old as on new items (Experiment 3b).

2. As in Experiment 2c, the further away in time the preview display is presented from T1 (i.e., the longer the SOA), the more efficient should search through the new, blue items become, simply because the old, green *H*s are better inhibited (Experiment 3a).

3. Detection of a probe dot on a new, blue item should also improve with SOA in Experiment 3a. If blue items are given priority under visual marking conditions, then probe dots on blue items should receive similar priority. No such improvement would be expected in Experiment 3b: Dot detection should be unaffected by SOA, as the dot is presented outside the attentional blink, and no priority should be given to either old or new items.

The probe dot detection paradigm also allows us to directly pit the spatial encoding account against the inhibitory account of how the attentional blink disrupts visual marking, as becomes clear from the following predictions:

4. According to the spatial encoding account, the closer in time we move the old distractors to T1 (i.e., the shorter the SOA), the worse the detection of probes on old, green *H*s should become, as the blink directly affects the spatial coding of the distractor and hence the localization of the dot (Experiment 3a).

5. In contrast, and perhaps rather counterintuitively, according to the inhibitory account, the closer in time we move the old distractors to T1, the better should subsequent probes be localized because a stronger attentional blink means that their locations are less strongly inhibited (Experiment 3a). Again, no such differential effects of SOA on probe detection were expected in Experiment 3b, in which old items should not be marked.

Predictions from a combination of the two accounts are harder to make, as any result depends on the relative strengths of both components. Therefore, depending on the direction of the outcome, we could conclude only that one component was present, not that the other component was absent.

As we expected the differences in dot detection to be relatively minor, we wanted to eliminate other sources of variance as much as possible, such as eccentricity effects and lateral masking. All search items were therefore placed at regular spacing on the perimeter of a virtual circle around fixation so that each item was placed at an equal distance from fixation (see Figure 9).

A final change was made to the overall design of the experiment. Because performance in the dual-task condition of Experiment 2 did not reach that of the single-task baseline, we decided to drop the single-task baseline altogether in Experiment 3. Instead, the effect of the attentional blink at short SOA (117 ms, inside blink) was measured against performance at the longest SOA (936 ms, outside blink) because Experiment 2 clearly indicated that visual search efficiency had stabilized by then. All other SOAs used in Experiment 2 were left out. Also, only one display size was used (Display Size 12).

Method

Participants. Thirty university students (13 male and 17 female, including 3 left-handed and 27 right-handed) participated for course credits or money. The average age was 22.9 years (range = 18–35 years). Twenty participants took part in Experiment 3a, and 10 took part in Experiment 3b.

Stimulus and apparatus. The experimental setup was largely the same as in previous experiments, with the difference that the search items were now placed on the perimeter of a virtual circle around fixation. The radius of this circle was 3.1°. Starting from a random position, six old items were positioned first around the circle, with regular spacing between them. The six new items then filled the spaces between the old items. On dot detection trials, a small, gray, circular disk (radius = 0.03°) appeared in the center of the bottom half of one of the search items. The green and blue letters were again roughly equiluminant, as determined by a flicker test on the experimenter.

Design and procedure. The sequence of displays was largely the same as in Experiment 2. However, there was no single-task baseline (see above), and there were only two SOAs, 117 ms and 936 ms, which we refer to as *inside blink* and *outside blink*, respectively. Only Display Size 12 (six old and six new items) was used. In Experiment 3a, on 20% of the trials, the visual search task was replaced by a dot detection task. Participants were signaled to change task by a 20-ms 1000-Hz beep presented 20 ms before the search display appeared. All search items remained visible, but a small dot appeared on one of them. The dot was equally likely to appear on a green item as on a blue item but never appeared on the blue *H* target. Participants had to click as soon as they spotted it and then, with the mouse, indicate where it was (by then, the whole display was replaced with position markers). As in the previous experiment, participants subsequently typed in what they thought was T1. Trial types were randomly mixed and presented in four blocks of 100 trials each (of which 20 trials were dot detection trials). Between blocks there was a break during which participants received feedback on their performance. Catch trials were not considered necessary in this experiment because on 20% of the trials (namely, the dot detection trials), the target should not be responded to anyway. Participants first received substantial practice by using the same step-by-step procedure as in Experiment 2. Because of the difficulty of the task, the first of the four blocks was also treated as practice. This left us with 120 trials for the blue *H* search task and 30 trials for the dot localization task (15 on old items and 15 on new items) for each SOA (inside blink, outside blink). It deserves mentioning, though, that the general pattern of results also held with the first block included. The design and procedure of Experiment 3b contained a few crucial differences. First, the dot appeared on every single trial, making dot detection the only task. Second, half the displays consisted of old, green *H*s followed by new, blue *A*s (green–blue condition), whereas the other half consisted of old, blue *A*s followed by new, green *H*s (blue–green condition). Experiment 3b thus served a number of purposes. First, it allowed for a direct replication of Watson and Humphreys's (2000) finding that inhibition was subject to top-down control. If a dot needs to be detected on every trial and it is equally likely to appear on an old as on a new item, then no marking should occur. Furthermore, any differences between dot detection for old and new items in Experiment 3a may have occurred because of low-level masking rather than top-down inhibition (e.g., old, green *H*s may interfere more than new, blue *A*s). If masking is indeed responsible, then similar differences should emerge in the green–blue condition of Experiment 3b, whereas the reverse should occur in the blue–green condition. Finally, Experiment 3b controlled for the differences in timing between T1 and the appearance of the dot. Any differential effect of SOA could in principle be due to a direct effect of the attentional blink on probe detection rather than to adverse inhibitory effects of old items. If so, performance in Experiment 3b should be very similar to that in Experiment 3a.

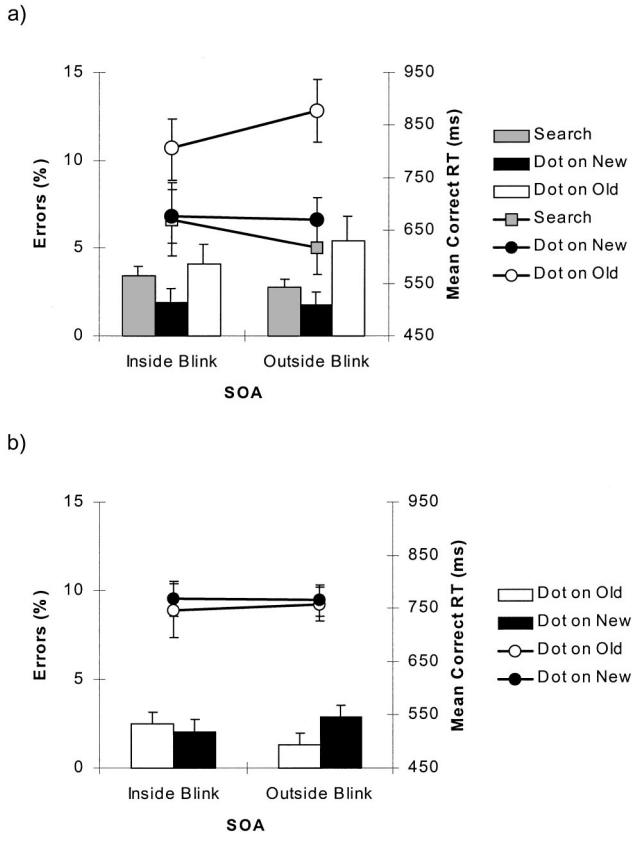

Figure 10. Reactions times (RTs) and error percentages for (a) Experiment 3a and (b) Experiment 3b. Columns represent the errors, and lines represent the RTs. Error bars represent one standard error. Dot on Old = dot appeared on old item; Dot on New = dot appeared on new item; Search = results for the search task (blue *H* target). SOA = stimulus onset asynchrony.

Results: Experiment 3a

Mean RTs and error rates for all conditions are shown in Figure 10a.

RTs. The recursive clipping procedure removed 3.5% of the RT data points. An overall ANOVA with trial type (blue *H* detection, dot detection on old item, dot detection on new item) and SOA (inside blink, outside blink) as factors revealed a main effect of trial type, $F(2, 38) = 22.1$, $MSE = 20{,}646$, $p < .01$, with RTs being fastest for blue *H* targets and slowest for probe dot targets presented on old, green items. The overall effect of SOA was not significant ($F < 1$). However, there was a significant Trial Type × SOA interaction, $F(1.7, 31.3) = 6.5$, $MSE = 6{,}142$, $p < .01$. When searching for a blue *H* target, participants were faster with the longer SOA (outside the blink) than with the shorter SOA (inside the blink; 671 vs. 618 ms), $t(19) = 2.1$, $p < .05$. In contrast, when searching for a probe dot on an old item, participants were slower with the longer SOA (806 vs. 877 ms), $t(19) = 2.5$, $p < .05$. SOA had no reliable effect on search for a dot on a new, blue item (677 vs. 670 ms), $t(19) < 1.0$, *ns*. This trial Type × SOA interaction also held for dot detection trials only, $F(1, 19) = 8.3$, $MSE = 3{,}672$, $p = .01$, confirming the opposite effects of the attentional blink on subsequent detection of dots on old versus new items.

Errors. As can be seen from Figure 10, error rates in the search and dot localization tasks followed the RTs ($r^2 = 0.72$). An ANOVA revealed only a main effect of trial type, however, $F(1.7, 31.3) = 6.8$, $MSE = 12$, $p < .01$, with significantly more errors being made with dots on old items than with dots on new items or blue *H* targets. The T1 miss rate was 7.5%.

Results: Experiment 3b

The recursive clipping procedure removed 0.9% of the RT data points. The remainder were entered in an ANOVA with display order (green–blue, blue–green), trial type (dot detection on old item vs. new item), and SOA (inside blink, outside blink) as factors. There were no effects involving the order of displays (all *p*s > .39), and Figure 10b therefore presents the data collapsed across colors. None of the other effects were significant, either (all *p*s > .40). Mean RTs were 746 ms (old, inside blink), 758 ms (old, outside blink), 768 ms (new, inside blink), and 765 ms (new, outside blink). Similarly, none of the error effects was significant (all *p*s > .19).

Discussion

How do the results match up with our working model of visual marking and the possible roles the attentional blink could play in it? Our first prediction was that the detection of dots on old, marked items should be slower than dot detection on new, unmarked items. This was clearly the case in Experiment 3a, as dot detection for old items was about 200 ms slower and less accurate than dot detection for new items. This stands in marked contrast to Experiment 3b, in which there was no difference in dot localization for old and new items (if anything, there was a small effect in the opposite direction). An ANOVA on the dot trials only, with experiment as a between-subjects factor, confirmed this, as it revealed significant Experiment × Trial Type interactions for the RTs as well as the error rates, $F(1, 28) = 23.9$, $MSE = 10{,}916$, $p < .01$, and $F(1, 28) = 7.1$, $MSE = 84$, $p < .05$, respectively; all other effects *ns*. As can be seen in Figure 10, overall dot detection RTs in Experiment 3b (around 750 ms) lay in between those for Experiment 3a (roughly 650 and 850 ms). In other words, the prioritization of the new items in Experiment 3a led to an overall dot detection improvement for new items, but this was paired with a cost for old items, indicating that such prioritization is achieved through a combination of inhibition (of old) and enhancement (of new) items.

Experiment 3b also shows that the differences in dot detection for old versus new items in Experiment 3a was not due to low-level stimulus differences (e.g., in color and onset). Furthermore, the absence of an SOA effect in Experiment 3b eliminates the possibility that the different intervals associated with the attentional blink were affecting probe detection directly. The present RT experiment therefore replicates Watson and Humphreys's (2000) earlier findings and offers further support for the idea that inhibition of old items is subject to top-down control. No inhibition is applied to old items when inhibition is actually harmful to the task at hand, for instance, when a dot must regularly be detected on them.

Our second prediction, that if more resources become available with longer SOAs, then visual marking should improve and so should subsequent search, was also supported. Indeed, we found that, as in Experiment 2c, search for the blue *H* became faster and more accurate when the old distractors had been presented outside the attentional blink period. This again suggests that the old distractors are more efficiently discarded when attentional resources are fully available, consistent with a visual marking account. Note, though, that in the present experiment the RT difference between the longest and shortest SOA was only 53 ms, which was considerably less than the 114 ms we found in Experiment 2c. Possibly, and quite likely, this may have been caused by the differences in display layout. The use of regular displays appears to have sped up search in general (compare the present RTs to Display Size 12 of Experiment 2c), leaving less room for preview benefits. Alternatively, after just having completed a dot detection trial, observers may tend to not mark the old items on the next trial. In any case, the difference between the two SOA conditions was expected to be reduced. However, the fact that there was a difference is clearly consistent with the theory of a resource-demanding inhibitory mechanism.

Our third prediction was that the pattern of performance for dots on new, blue items should follow that of the blue *H* target, following the rationale that if blue items gain priority in selection, so should dots in the same location. This was not the case. Figure 10a shows there was only a minute decrease (7 ms) in the dot detection condition. Although somewhat puzzling, this result corresponds to earlier findings by Watson and Humphreys (2000), and we therefore believe it is real. Watson and Humphreys (2000), using percentage correct as a measure, found that the accuracy difference for probes on green versus blue items was greater in the preview condition than in the conjunction baseline. However, and important for the present results, the effect was almost entirely accounted for by a drop in accuracy on green items in the preview condition. In other words, probe detection for blue items was as good in the conjunction condition as it was in the preview condition, and so here too visual marking had no effect. One possible explanation is that blue (new) items already receive maximum enhancement in all conditions and that further prioritization can only be achieved through inhibition of old items. This would be consistent with a visual marking account.

Fourth, we predicted that if the attentional blink affects the spatial encoding of the old distractors, then a subsequent probe dot presented on one of these distractors should be more difficult to localize, as the visual system does not know where the item is. When resources become available again, localization should improve with SOA. However, we found the opposite: Probe dot detection was better when the distractors had been presented inside the blink period and became worse with longer SOA. This goes against the spatial encoding account being the sole explanation of the decreased efficiency of visual marking under attentional blink conditions, although it does not rule it out. An important assumption underlying our predictions was that the probe dot is somehow tied to the representation of the old items (i.e., when an old item becomes suppressed, a dot appearing on it becomes suppressed too). Although the data support this assumption, such coupling need not be on a spatial basis. In other words, old items may be poorly spatially represented, but this does not necessarily extend to the probe dot.[4]

Instead, the results are more in line with our fifth and final prediction: According to the inhibitory account, probe detection should actually improve with shorter SOA, as the inhibition will deteriorate. Both RTs and errors followed exactly this pattern, suggesting that the visual system does become less effective in inhibiting distractors when hindered by an attentional blink.

Obviously, the results could also stem from a combination of the two effects, in which the modulation of the inhibitory stage is just the stronger of the two. The fact that the displays in Experiment 3 were highly regular may have contributed to the relative irrelevance of the spatial encoding process. Alternatively, the two effects may have different time courses, with spatial encoding only affected early in the blink period, after which it recovers quickly. The inhibitory process, on the other hand, may be affected in a later stage and recover more slowly. Because we measured probe detection only after the blink period, we may have missed out on any early spatial encoding deficits and measured only inhibitory effects.

The absence of a spatial effect in Experiment 3a also opens the possibility that part of the inhibition in visual marking is actually not linked to individual locations. Instead, inhibition may be applied to various nonspatial representations. As mentioned in the beginning of this article, recent studies on visual marking have suggested that inhibition may be linked to properties other than location. For instance, Watson and Humphreys (1998) have shown that when moving, green distractors are being ignored, newly appearing green distractors also fail to interfere with search. Recent experiments in our lab have confirmed this finding further with static displays. Singleton distractors, presented in the new set, became less interfering the more they resembled the old items (Olivers & Humphreys, 2001b). This suggests that some inhibition is being transferred from old to new items on the basis of shared properties. Watson and Humphreys (1998) proposed that for some displays, inhibition may be feature based, for example, applied to an entire color map. Any item exhibiting that feature will then be effectively ignored. If location information is difficult to maintain (as with complex motion displays), then feature-based inhibition may be the only means available for ignoring distractors (Olivers et al., 1999). If old and new items cannot be distinguished by a single feature, then location-based inhibition is more appropriate, and if both types of information are available, then either or both can be used as appropriate representations for the inhibitory process to act on. We tested this hyphothesis in Experiment 4.

Experiment 4: Further Support for Nonspatial Inhibition

In Experiment 4 we adopted a somewhat different procedure to measure the amount of inhibition remaining after distractors had been presented inside or outside the blink. As outlined in Figure 11, trials were largely the same as in Experiment 2c: They started with an RSVP stream containing a T1 target, followed by a preview display containing green *H*s, followed by a search display containing blue *A* distractors and a blue *H* target. Participants had to localize the target in a pointing display, which in turn was followed by the cue to identify T1. The difference between the studies lay in the addition of a second

[4] We thank Adriane Seiffert for pointing this out.

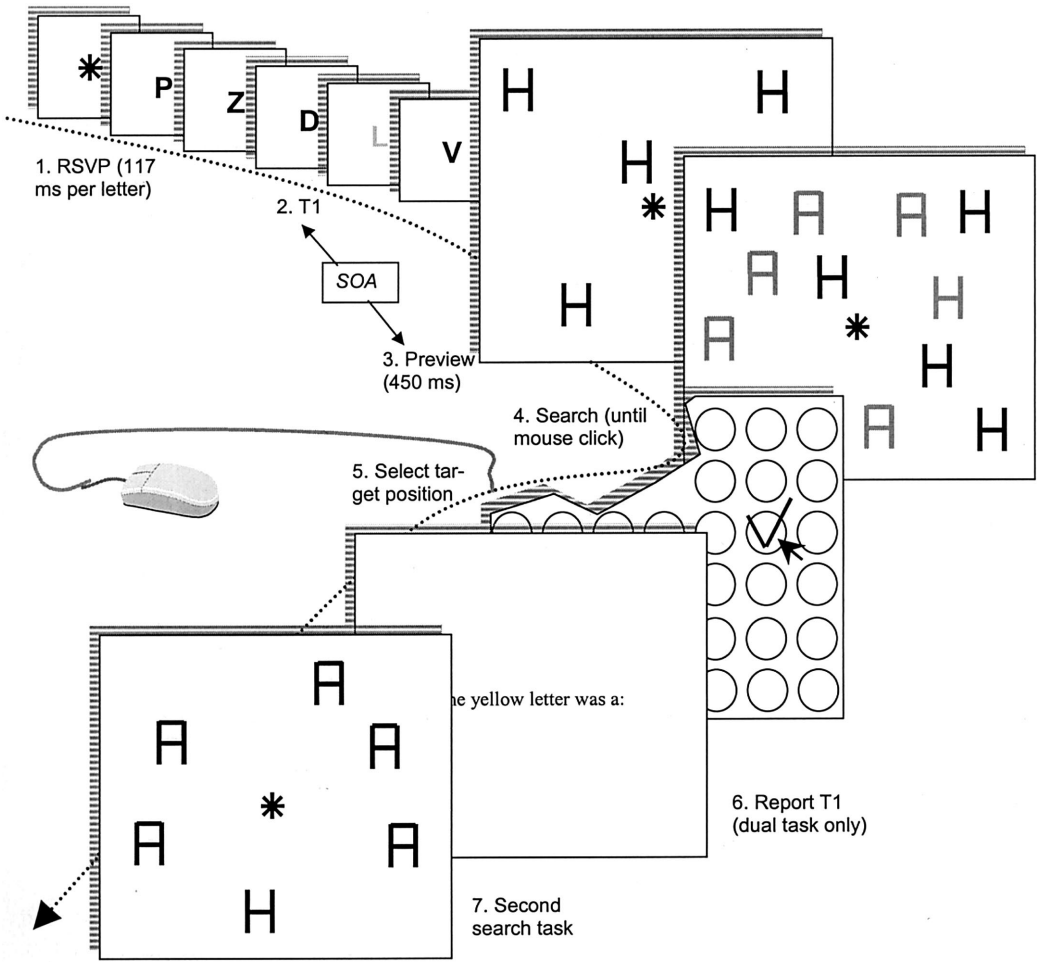

Figure 11. Part of the procedure of Experiment 4. The sequence of events was exactly the same as in Experiment 2c except for appearance of the second search display (Step 7). This display consisted of green *A* distractors and a green *H* target (identical to the green *H*s in the preview display, Step 3). In the same-location condition, the second search items appeared in the same locations as the previewed items. In the different-location condition, they appeared in randomly selected different locations. RSVP = rapid serial visual presentation; T1 = first target letter; SOA = stimulus onset asynchrony.

search display just after the T1 response was given. This second search display consisted of green *A*s and a green *H* target, which had to be localized just like the blue *H*. We hypothesized that the more effectively the green *H*s are suppressed in the preview displays, the slower the detection of the green *H* target in the second search display should be. Thus, previewed green *H*s presented outside the attentional blink should lead to better selection of the blue *H* target in the first search display but to worse selection of the green *H* target in the second search display. In contrast, green *H*s presented inside the blink should lead to worse selection of the first target but better selection of the second target.

We also varied the spatial layout of the second search display. In the *same-location* condition the items fell in exactly the same positions as the previewed items, whereas in the *different-location* condition the items were positioned at random. This manipulation was added in an attempt to differentiate further between location-based and object- or feature-based effects. If the inhibition in visual marking is (partly) location based, then any differential blink effects may be expected to be greater in the same-location condition than in the different-location condition.

Note that our procedure resembles that of the negative priming paradigm. In the negative priming paradigm a target on trial *n* may have been a distractor on trial $n - 1$, and typically a cost is observed relative to an unrelated previous distractor. This cost has been attributed to inhibitory processes operating on the distractor (e.g., Tipper, 1985). We return to possible links between visual marking and negative priming later.

Method

Participants. Twenty-one participants (2 male and 19 female, including 5 left-handed and 16 right-handed) participated for either course credits

or money. The average age was 20.5 years (range = 17–29 years). Participants were a priori excluded from analyses if they had noticed the spatial relationship between the preview display and the same-location condition of the second search set. Without this rule there would be the risk that observers may have used the preview display as a cue for the second search display because it carried information about the possible target position. Only one participant was substituted because of this.

Stimulus, apparatus, design, and procedure. Equipment, stimuli, and setup were identical to that in Experiment 2c. The design and procedure were also very similar, with a few exceptions. As in Experiment 3, there was no single-task baseline, there were only two SOAs—117 ms (inside blink) and 936 ms (outside blink)—and there was only one display size (12). Furthermore, the stimulus sequence was extended with a second search display, which occurred after the T1 letter was entered. For this purpose, the T1 response display was followed by a 750-ms blank display. Subsequently, a fixation cross would appear for 500 ms, followed by the second search display. The new display thus seemed like a new trial with a new display. The second search display consisted of green box-shaped *A* distractors together with a green *H* target, which participants again had to localize by clicking a mouse button and then pointing to the target's position in a separate pointing display. Unpublished experiments in our lab have shown that preview effects may last for at least 2.5 s, warranting our expectation that some inhibition may transfer to the second search task, which was presented relatively long after the initial preview. The layout of the second search set was varied so that the items fell in the same positions as the previewed distractors (same-location condition) or in randomly selected different positions (different-location condition). The second search display timed out after 5 s. If an error was made during any of the three stages of the trial (first search task, T1 report, or second search task), then the trial was repeated by random insertion in the remainder of the block. Participants first practiced the task in stages and then completed one block of at least 100 correct trials. Of these 100 trials, 50 contained a preview display presented inside the blink and 50 contained a preview display presented outside the blink. In each blink condition, 25 displays were followed by a second set of search items in the same location as in the preview, and 25 were followed by items in a different location. All conditions were randomly mixed.

Results

RTs. Figure 12 shows the RT data for each SOA and task. The recursive clipping procedure resulted in 2.4% of the data points being removed. An ANOVA with SOA (inside blink, outside blink) and task (first-set search; second set, same location; and second set, different location) as factors revealed significant main effects of SOA, $F(1, 20) = 4.5$, $MSE = 3{,}577$, $p < .05$, and task, $F(2, 40) = 16.2$, $MSE = 17{,}028$, $p < .001$. RTs were faster overall when preview distractors were presented outside the blink, and performance on the second-set search was faster overall than performance on the first-set search. There was also a significant Task × SOA interaction, $F(2, 40) = 19.4$, $MSE = 3{,}931$, $p < .001$. As can be seen from Figure 12, performance on the visual marking task improved with increasing SOA, whereas it deteriorated on the second search task. To test this interaction further, the analyses were split into a *t* test (Fisher's LSD) for the visual marking task (first set) and a two-way SOA (inside, outside) × Spatial Layout (same, different) ANOVA for the second-set search. The *t* test confirmed the finding that RTs for the first set improved when distractors were moved outside the blink (*M*s = 875 vs. 754 ms), $t(20) = 4.80$, $p < .001$. Most important, the ANOVA confirmed that performance on the second-set search deteriorated with SOA (*M*s = 661 vs. 688 ms, averaged across spatial layout), $F(1, 20) = 6.6$, $MSE = 2{,}251$, $p < .02$. However, there was no effect of spatial layout, nor was there a reliable Spatial Layout × SOA interaction (*p*s > .53).

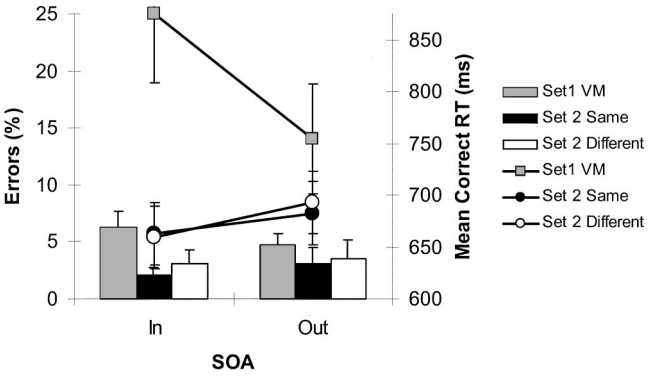

Figure 12. Reaction times (RTs) and error percentages for Experiment 4. Set 1 VM = search results of the visual marking displays; Set 2 Same = search results of the second set with the same spatial layout as the preview; Set 2 Different = results for second search displays but with different spatial layouts than the preview. Error bars represent one standard error. SOA = stimulus onset asynchrony.

Errors. Figure 12 suggests that the error pattern followed the RTs closely. This was confirmed by a correlation of .96 between the RTs and error percentages. An ANOVA pointed toward a reliable effect of task, $F(2, 40) = 5.0$, $MSE = 20$, $p = .01$. More errors were made on the visual marking task than on the second search task. No other effects were significant (*p*s > .20).

Discussion

Experiment 4 replicates and extends the findings of Experiment 3. Moving the green *H* distractors outside the blink improved the detection and localization of a blue *H* target but led to decreased performance associated with the green *H* itself. In Experiment 3 this was measured through detection of a dot presented on one of the green *H*s; in Experiment 4 this was measured through the detection and localization of the green *H* in an additional search display at the end of each trial. Search for a green *H* was on average 27 ms slower when old, green *H* distractors had previously been presented outside the blink relative to inside the blink. Thus, the results of Experiment 4 indicate once more that old distractors are inhibited and that this inhibition is affected by removing attentional resources.

In addition, the modulation was no weaker (if anything, it was a bit stronger) when the displays in the second search task had a different layout than that of old distractors in the first search task, relative to when they had identical spatial layouts. We therefore conclude that at least part of the inhibition is not tied to the old distractor locations. Instead, suppression may be linked to nonspatial object properties such as color or object identity.

Note again, though, that the failure to find any effects of spatial layout does not mean that there is no inhibition tied to old distractor locations. Our measure may simply not have been sensitive enough to measure location-based suppression. For instance, the abundant occurrence of blank as well as filled screens (e.g., the pointing and T1 response displays) between the preview and second search displays may well have wiped out any location-

based suppression (cf. Horowitz & Wolfe, 1998; Takeda & Yagi, 2000; Watson & Humphreys, 1997). Alternatively, location-based suppression may be only short-lived and may even turn into facilitation. For instance, observers may develop an implicit memory trace for the display layout, which helps them in finding the target in a later stage when search items are presented in identical layouts (cf. Chun & Jiang, 1998). The longer the preview display is presented outside the blink, the better the implicit memory trace and the more facilitation results from it. The lack of an effect of spatial layout in our displays could thus be due to a mixture of facilitation and inhibition.

As in the negative priming paradigm, here the second search target was presented on what could be seen as a separate trial, and it was related to the previewed distractors. The difference between this and the typical negative priming task is that here the target and distractor were always related, and the strength of the adverse effect of previous distractors was modulated by the attentional blink. The relationship between the present results and those obtained with negative priming opens the interesting possibility that part (but not all; see Watson & Humphreys, 1997) of the inhibitory processes involved in visual marking may be the same processes operating under negative priming conditions. In further support of this, studies of negative priming have shown that the inhibition can be, at least partially, object based (e.g., Tipper, Brehaut, & Driver, 1990) and that it is susceptible to attentional load (Lavie & Fox, 2000). We return to this point in the General Discussion.

General Discussion

We used the attentional blink as a tool to investigate two important aspects of visual marking: (a) whether visual marking involves the top-down inhibition of old items and (b) whether this inhibitory mechanism requires attentional resources. The present experiments provided evidence for both.

Experiment 2c showed that presenting old distractors inside an attentional blink led to their subsequently being included in the search set. Gradually moving the distractors outside the blink period resulted in more and more efficient search through the new set. Since the new set was presented only after participants had recovered from the attentional blink (400–500 ms, as confirmed by the control conditions of Experiments 2a and 2b), we concluded that the attentional blink must have taken away important resources from the old items—resources, we hypothesized, that are necessary to actively inhibit distractors in anticipation of the search set. Experiment 2 thus demonstrated the task constraints operating on visual marking. When the visual system is highly occupied with a secondary task, it postpones or abolishes the setup and/or utilization of an inhibitory template. Therefore, items that would normally be ignored remain active and have an adverse effect on visual search through new items. Experiment 3a showed a reversed effect. If the task is to detect a dot on a minority of trials and this dot appears on one of the old items, then its detection and localization suffers relative to dots presented on new items. However, presenting the old items inside the blink has an advantageous effect: Subsequent dot detection improves for old items. When the task changes and dots are detected on every trial, the difference between old and new items disappears completely (Experiment 3b). This provides further support for the involvement of inhibitory processes, which are subject to top-down settings and limited-capacity resources. Furthermore, because Experiment 3 involved the explicit localization of the probe dot, the results suggest that, within our model of visual marking, the attentional blink affects the inhibitory stage more than the spatial representation stage.

Experiment 4 provided evidence that the inhibition of old distractors has at least a strong feature- or object-based component and is not tied solely to old distractor locations. Previewed distractors presented outside the attentional blink led to slower search times and more errors when they became the target of a search task presented shortly after, regardless of whether the search display had the same spatial layout as the preview display. These results suggest that some of the inhibition may be object-, feature- or identity-bound, in accordance with earlier studies on negative priming and on visual marking with moving items.

These experiments offer strong support for large parts of our model of visual marking (Figure 1). In anticipation of relevant target information, irrelevant distractors are inhibited. Furthermore, this inhibition is task dependent and resource limited, aspects which are indicative of a top-down process. However, evidence for the location-based implementation of the inhibition has been rather thin. Experiment 3 suggested that the spatial representation of the distractors was less affected than their suppression, and Experiment 4 failed to demonstrate stronger inhibition for spatially identical layouts relative to different layouts. Possibly, location-based inhibition does not play as strong a role as we envisaged, and other types of inhibition may be equally, or more, important (cf. Olivers et al., 1999; Watson & Humphreys, 1998). Alternatively, the attentional blink may not affect spatial coding at all, or may affect it only in an early stage of the blink, allowing for the spatial representation to be recovered before we are able to measure any detrimental effects (notice that the dot detection and second search tasks in Experiments 3 and 4 were always presented relatively late). It is interesting to note that, in a somewhat different experiment, we did find effects of the attentional blink on spatial encoding (Olivers & Humphreys, 2001a). Instead of a visual marking task, we used a spatial cuing task in which we gave participants a brief and masked preview of the future locations of the target set rather than the distractor set (as in the normal preview paradigm). The mask was introduced to prevent recovery of spatial processing later during the blink. The preview consisted of a variable number of cues and therefore varied in its predictability of the target positions (i.e., the more cues, the more uncertainty about the target position). We measured the observers' capacity to process and retain these cues across the blink period and found that, on average, spatial capacity was halved inside the attentional blink. Apparently, taking away attentional resources results in cues not being appropriately tagged, or it limits their access to VSTM. Similar spatial processing restrictions may operate on visual marking under attentional blink conditions. Our main conclusion therefore is that inhibitory effects are present and they are under top-down control. Whether the buildup of a spatial representation is equally under top-down control remains an issue for further investigation.

Implications for the Attentional Blink

Our results are also important with respect to the attentional blink. Several studies combining priming and RSVP have shown that non- or misidentified items presented inside the blink can still facilitate processing of identical or related targets at a later stage (a

positive priming result; Luck, Vogel, & Shapiro, 1996; Maki, Frigen, & Paulson, 1997; Shapiro, Driver, Ward, & Sorensen, 1997). Maki et al. (1997), using a semantically related prime, showed that the strength of priming did not alter with increasing lag between T1 and the prime. In other words, the prime was as effective when presented inside the blink as when presented outside the blink. The present experiments address a closely related issue, as we asked to what extent items inside the blink still interfere with, rather than facilitate, subsequent target processing. As suggested earlier, Experiment 4 (and, to a certain extent, Experiment 3) can especially be regarded as manipulating a form of negative priming, as previously presented distractors resulted in slower search times when they became targets. In contrast to Maki et al., we found that these adverse effects were strongly modulated by the strength of the attentional blink. Distractors presented inside the blink resulted in more interference than distractors presented outside the blink. This suggests that the attentional blink may allow for a considerably high level of representation of stimuli (leading to facilitation even on the semantic level) but that the suppression of these representations is severely affected. This may mean that the inhibitory mechanisms are either operating in an even later stage in the visual stream or are generated elsewhere and thus operate externally on visual processing.

The priming studies strongly suggest that items presented within the attentional blink period are active and that the blink allows for considerable amounts of processing. As a consequence, distractors presented inside the blink remain active too. However, there is another possibility. Perhaps items do not become active inside the blink but are simply put on hold until the blink is over (Chun & Potter, 1995; see below), and only then do they lead to facilitation or interference. Thus, instead of stating that the old distractors stayed active, it is perhaps safer to conclude that they were not successfully deactivated. Future research will be necessary to address these questions.

There are further implications for models of the attentional blink. One such model is the VSTM interference model of Raymond and colleagues (Raymond, Shapiro, & Arnell, 1995; Shapiro & Raymond, 1994). According to this account, the attentional blink is caused by too many items entering VSTM, including T1, T2, and often the intermediate distractors. The items within VSTM will compete for response retrieval, a competition usually won by T1 because of its status as the first task and its distinctive color. As a consequence, T2 retrieval will suffer. One prediction from the interference account is that the more items enter VSTM, the stronger the interference will be, and hence the greater the costs associated with the attentional blink. Isaak, Shapiro, and Martin (1999) found evidence in favor of this prediction, be it only on the conceptual level rather than on the feature level (i.e., letter distractors interfered more with letter targets than did nonletter distractors even though the nonletters were visually more similar). The VSTM interference model appears, at least in part, to be compatible with our results. Much of the interference from the previewed distractors may be caused by too many distractors gaining access to VSTM, resulting in interference with target selection. On the other hand, within our current model of visual marking, some attentional processing (and thus access to VSTM) of the old items may actually be required for the inhibitory template to be set up, and the more old distractors enter VSTM, the more they can be deprioritized. Also, it is most likely that the interference from the old distractors occurs on a feature level rather than on a conceptual level, contrary to what Isaak et al. (1999) found. Finally, in our experiments, T2 was replaced by a preview display to which no response was required. Hence, it is unlikely that there was any competition for response retrieval within VSTM during the blink period.

Instead, our data seem most consistent with a simpler and more general model outlined by Shapiro, Arnell, and Raymond (1997), combining aspects of many other models of the attentional blink (Chun & Potter, 1995; Jolicoeur, 1998; Jolicoeur & Dell'Acqua, 1998; Seiffert & Di Lollo, 1997). Here we quote the first two tenets of this unified model:

> (1) As a result of the T1 mask, increased attention is required to enable T1 to reach a level of awareness sufficient for report.
>
> (2) As less attention is available for T2, by virtue of T1's demands, T2 cannot be consolidated into a durable storage sufficient for report. This leaves T2 vulnerable to decay and/or object substitution from a variety of stimulus sources. . . . In spite of the inability to report T2 with a high degree of accuracy during this interval, T2 is processed to a level of semantic awareness. (Shapiro et al., 1997, p. 293)

In our experiments, too, T1 takes up most of the attentional processing. Post-T1 stimuli may be processed up to the semantic level (cf. the negative priming aspects of Experiment 4), but too few attentional resources are available too late to inhibit the old items. When resources are released again, the new items are due to arrive, and both old and new will be selected together. As a consequence, search is slowed.

Selective Attention and Resource Limitations

One of the major findings in the present study was that the more the attentional system is engaged with the RSVP task, the more irrelevant information will subsequently interfere with visual search. On the surface this conclusion seems to stand in contrast to earlier proposals by Lavie (Lavie, 1995; Lavie & Cox, 1997; Lavie & Tsal, 1994), who suggested the opposite: The more attention is involved in a perceptual task, the less irrelevant information will interfere. Lavie and Tsal (1994) proposed that the capacity available for visual processing will determine the locus of attentional selection. When observers are engaged in a task with low attentional load, they will have resource capacity to spare. Lavie (1995) suggested that this spare capacity must be spent and will inadvertently be directed toward distractors, which may in turn interfere with the central task. In contrast, with a very demanding task, all attentional capacity will be used up, leaving no resources for distractors (Lavie, 1995; Lavie & Cox, 1997). In our experiments we found the opposite result. The more observers were engaged in the RSVP task, the stronger the distractors subsequently interfered with search.

However, our results are by no means incompatible with Lavie's (1995) hypothesis. First, assuming that the RSVP task does indeed impose a perceptual load on the system, there remains one obvious difference between our procedure and Lavie's (1995), namely, the temporal order of the displays. The perceptual load hypothesis appears especially powerful when targets and distractors are presented simultaneously. In this case, if the target requires more processing, the distractors receive less processing. In a typical preview procedure, however, the target information appears only after the distractors. In this case, efficient processing of the target crucially depends on the distractors already having been processed

first. If this preprocessing is prevented by a central task (e.g., the RSVP task employed here), then both distractor and target sets will receive a share of attentional processing during search.

Our results are even better reconciled with Lavie's (1995) hypothesis if we assume, as suggested earlier, that the RSVP task does not so much impose a perceptual load on the visual system, but more a central, cognitive load, affecting working memory (see, for instance, Vogel & Luck, 1999, for physiological evidence).[5] Lavie, in a 2000 review, proposed that working memory is needed to

> maintain current priorities and thus ensure that low-priority items can be suppressed. Contrary to the predicted effect for perceptual load, however, increasing the load on these higher mental functions will drain the capacity available for active control and result in more, rather than fewer, intrusions from irrelevant distractors. (pp. 175–176)

This idea fits well with our present findings and supports the hypothesis that visual marking involves the top-down controlled suppression of distractors. Lavie (2000) further argues that such top-down control of selective attention within working memory is likely to reside (at least in part) in the frontal lobes (cf. Kastner & Ungerleider, 2000). We recently conducted a functional magnetic resonance imaging study on visual marking, which showed, among other areas, increased activation in the bilateral prefrontal cortex, around the middle frontal gyri and superior frontal sulci (Brodmann Areas 6 and 8; frontal eye fields), and in right superior parietal areas (Brodmann Area 7). These areas have previously been attributed with the functions of spatial working memory and the top-down modulation of spatial selection in attention (Corbetta et al., 1998; Courtney, Petit, Maisog, Ungerleider, & Haxby, 1998; Haxby, Petit, Ungerleider, & Courtney, 2000; Kastner, Pinsk, De Weerd, Desimone, & Ungerleider, 1999; Rowe, Toni, Josephs, Frackowiak, & Passingham, 2000). It is therefore possible that exactly these areas are affected by the attentional blink, resulting in a loss of inhibition. In further support of this idea, Marois, Chun, and Gore (2000) also found a right frontoparietal pattern of activation under attentional blink conditions.

[5] We thank Yuhong Jiang for pointing this out.

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